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CONTRIBUTIONS
TO
PHYSICAL AND MEDICAL
KNOWLEDGE,

Principally from the WEST of ENGLAND,

COLLECTED BY

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THOMAS BEDDOES, M. D.



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INTRODUCTION.

In reflecting at various times on the necessity and the means of acquiring medical facts, a few ideas have occurred to me such as I have not met with in conversation or in books. I hope they will not be thought misplaced at the head of a publication like the present.

Of human knowledge the most important part is that which relates to human nature. He who should undertake to demonstrate or to enforce this truth would engage in the most thankless of offices. His arguments and his rhetoric would fall infinitely short of the persuasion that already prevails.

The science of human nature is altogether incapable of division into independent branches. Books may profess to treat separately of the rules of conduct, of the mental faculties and the personal condition. But the moralist and the metaphysician will each to a certain point encroach upon the province of the physiologist.—Every code of morals must ground its precepts on a comprehensive view of the laws that regulate feeling, and deliver

the conditions of an offensive and defensive league, having for its object the well-being of individuals. Without accurate ideas therefore of the causes that affect the personal condition of mankind, how is it possible to conceive any progress in genuine morality? And will not every addition to this branch of knowledge necessarily tend to purify morals—that is, to introduce into the social compact covenants more beneficial to the parties? Without reference to the body, it is equally impossible to unfold the nature of the mind. Physiology therefore—or more strictly *biology*—by which I mean *the doctrine of the living system in all its states*, appears to be the foundation of ethics and pneumatology. If we attend to common conversation, we shall find, that few questions concerning morals and none at all concerning intellect can be discussed at any length without a formal appeal to physiological axioms.

With this analysis it would be curious to compare the observances of different ages and nations. Few communities, in any degree civilized, are without provisions for inculcating principles of conduct. In polished societies more than mere preceptive morality is attempted. The professors of liberal education undertake at least to teach the theory of the rational faculties. But here general education stops. No seminary descends to the prime, fundamental, sure part of self-knowledge. A young person may come out into the world ALL ACCOMPLISHED, and not have the smallest conception of what goes on within his own frame, unless he has by chance heard of the circulation of the blood. To

their proper interior selves preceptor and pupil are alike strangers and alike indifferent. Were it a consecrated maxim that the fashion of apparel is more intimately connected with human welfare than the art of preserving the functions from disorder, education need not be different from what it now is.—Will it be said that I do the age injustice, because the care of the person was never carried so far as at present? But this care, I conceive, is misnamed *care of the person*. To the person it is probably oftener injurious than beneficial. It refers to the beholder's eye, not to the feelings of the subject on which it is bestowed. So it can but make the surface shine, it leaves the substance below free to rot.

Almost every man's sensations may convince him what a penalty this order of things imposes. It is a penalty levied not upon the sick only, but upon that countless multitude who though they resort to no physician, continually carry about with them a comfortless consciousness of existence.

How those with whom it rests may be induced to open to the rising generation the most beneficial pages in the book of knowledge is a question which I have considered with some care elsewhere—(*Lecture introductory to a course of popular instruction on the constitution of the human body*. JOHNSON). But among the smaller means of rendering education an apprenticeship to happiness and qualifying people to take themselves into their own keeping, I number the union of *anthropology* with other branches of philosophy in the same publication.

The latter is the proper introduction to the former; it enjoys universal currency and may be instrumental in procuring for the other the same advantage. Accordingly in announcing the present collection, I said: "It is not proposed to confine the work strictly to medical papers. With the philosophy of inanimate nature which bears in so many points upon his art, no practitioner of medicine should be unacquainted, Nor does any thing seem more demonstrable than that every liberally educated individual should be initiated into the philosophy of animated nature. An extensive series of experiments in the first volume will furnish a striking example of the connection between these branches of knowledge. And the propriety of keeping them together ought perhaps on all occasions to be held up to contemplation."

My opinion concerning the utility of this association may be accounted fanciful. Nor will I defend it with much warmth.—The grand expedient for rendering physiology popular and medicine certain, is to enlarge our stock of observations on animal nature. Before the invention of printing, there could be but one practicable way of accomplishing this purpose. Individuals were obliged to commit singly to writing what each had seen and thought. When there was not matter enough for a proper volume (*justum volumen*), the ideas perished with him who had conceived them. Information therefore accumulated slowly. In later times a happy expedient presented itself. Periodical publications for preserving and embodying short and otherwise fugitive pieces were set on foot. To these repositories

we owe much of the superiority of the modern over antient medicine. To establish them on the one hand, and to supply them on the other, is perhaps the utmost that can be accomplished by the co-operation of the members of the profession. But a more effective union of powers is possible. If whole communities could but relinquish petty interests to join in the great work of forwarding the knowledge, and by consequence diminishing the sufferings, of human nature, the treasure of physiological facts would be easily doubled in less than twenty years.

As the *manner* in which the labour of so many ages may be surpassed in this short space of time appears to me extremely simple, I shall present a sketch of it here. Simple as it is however, I am not so visionary as to expect to see my method tried. The history of all useful designs is nearly similar. Though ridiculed on their first proposal by the shallow-minded slaves of habit, they are approved at least in principle, and commended, by the intelligent. By degrees the idea becomes generally understood and goes on gathering suffrages in silence. At length, the fullness of time arrives. Its execution is proposed, and mankind are surprized to find their opinions united in its favour.

My leading principle is to *provide for the most perfect possible ascertainment, and entire publicity of all the phenomena, occurring in charitable establishments for the relief of the indigent sick.* I do not at present enter into any enquiry respecting these establishments. On a particular occasion I endeavoured to throw out some suggestions for extending their benefits without increasing their expence; and at

some future period I may revise and republish my paper, which on its first appearance was calculated but for provincial circulation.

The medical functionaries of hospitals should be required at fixed, perhaps monthly, periods to furnish an account of their respective departments, particularly noticing such phænomena as should appear to them instructive or singular.*

* Since my MS. has been in the printer's hands, I have learned that in the case of cancer, some surgeons at Manchester acknowledge the propriety of making the facts occurring in hospitals notorious. In a memorial addressed to the trustees of the M. hospital, they observe, "that if a journal were kept in the manner they propose, *and to be open to general inspection*, much good would arise to the objects of these institutions, and general benefit to the community. We have therefore agreed to keep an exact account of each case of cancer which shall come under our care, in which shall be recorded a faithful history of the disease, &c. &c. &c." The proposal was adopted. (*Simmons on the Cæsarean operation* 1799. *Vernor and Hood*. p p. 74—5.). It is, I presume, impossible for the human imagination to supply any specious reason for the above limitation of the principle. As medicine fails in so many cases besides cancer, there is just the same reason for "recording a faithful history of the disease, with its attendant circumstances; the effects of medicines; and of operations when necessary; together with all the collateral helps to be gained by an enquiry into constitutional habits and diseases" There is also in other instances just the same reason for giving publicity to the information which all this care shall have accumulated, in order that as many minds as possible may be set at work to detect the means of ease and health. In the part of the above quotation printed in italics, I am aware that *general* may mean *confined to the hospital surgeons*. When the rules of hospitals are framed according to the system, most conducive to the good of the patients they receive, and of the benefactors who uphold them, the word will be employed in its usual unlimited signification.

To these meetings all the practitioners of the place and neighbourhood, together with subscribers to the charity, should have free access. When the statement contained any thing uncommonly interesting, a commissioner or committee of verification should be appointed to examine the circumstances. In cases not admitting of delay, the attending physician or surgeon should call in one or more commissioners during the intervals of the sittings. It is of course that the facts thus acquired should be subject to the remarks of the parties present, and that the more select should be given to the public in some commodious form.

In large and capital cities, the establishments that are supported by general contribution might thus be rendered most effectually subservient to the general good. There, the transactions of many hospitals might be brought under review at once, and contrasted. There also, will be the greater chance that particulars in the reports shall suggest discoveries to some one among a numerous audience, though the very same particulars, for want of necessary previous associations, should be utterly lost upon the reporters themselves; just as sparks fail to kindle a blaze when they fall upon incombustible materials. So intimate likewise has science rendered the connection between the organic and inorganic kingdoms of nature, that the unprofessional philosopher, well apprized of the desiderata of medicine, may be sometimes more in a capacity to supply them, than the unphilosophical practitioner.

After considering the stake which society has in medicine; how often in a man's life it may, according to its

power and administration, wound or soothe his personal feelings and his sympathies; let him imagine himself present at one of these sittings where the business is carried on with a spirit adequate to its importance. What motives for self-congratulation, and for congratulating his whole species, will the scene before him offer! the art of most immediate and most universal concern, brought out of that darkness in which none can distinguish whom it preserves and whom it destroys! its doubts solved! its contradictions reconciled! the causes of phenomena, where, cause and existence are at issue, detected! light and order suddenly spread through trains of ideas that had long been vainly struggling in the minds of the ingenious! the stores of the most knowing augmented! the powers of the most acute sharpened! the interest of all classes promoted! the fortunate son of *Æsculapius* retiring with better informed judgment to the mansions of the opulent! the humbler practitioner carrying away comfort to the peasant and the pauper! All things considered, I know not if those assemblies which fix the destiny of distant nations and unborn generations would appear in the eye of humanity more august, or be found more capable of inspiring exalted sentiments.

Europe abounds in private medical societies. It abounds in public philosophical societies, pursuing with success every species of knowledge, but the knowledge of man. These are the disjointed elements of the association here suggested. They do not require a great deal more than to be united. The leisure of the extra-professional members,

their ingenuity, and (what in physiological researches is often equally important) their wealth would enable them to pursue many trains of experiment to which the means of the professional members would be in every way unequal. The school of HALLER, (from whose researches, together with those of his pupils, modern celebrated doctrines derive their principles,) shews how much may be accomplished in a short time, when there is any person or occasion to inspire energy. Many similar examples would have occurred in the history of our art, if all who have professed teaching had practised thinking. But, according to my system, it will be nearly indifferent to the progress of physiology, by whom lecturers' chairs, or medical offices, are filled. All the combinations of which the state of knowledge is at any time capable, would be formed by genius, and prosecuted by industry.

Many would undoubtedly shrink from such an effort of attention and such a test of ability as I require.* To be physician to an infirmary is often more a title than a function, and retained out of jealousy or pride. Let it not however be forgotten that I suppose my design impracticable till a discovery

* In Germany there are many small hospitals and dispensaries of recent date. The persons who preside over them have almost universally considered it as a duty to publish this part of their practice, that the benefit might extend beyond the immediate objects of the charities. I have beside me half a dozen such publications and suppose I could name twenty more. It appears that the continental physicians are emancipating themselves from the shackles of old medical theories, that they are fixing their undivided attention upon the course of nature, and that for them a new and auspicious æra in medicine is beginning.

in physiology shall be capable of exciting as warm sensations as a ministerial harangue. In such a state of national feeling nothing will appear a labour of supererogation. As medical philosophy gains more of the public attention, medical practitioners will become more and more the devoted servants of their art. Such as is the community, such will ever be medicine. The more intelligence in one, the less quackery, the less intrigue, the less helplessness in the other. Public participation would rouse the ingenious from their indolence, and deprive the incapable of that credit which the public ignorance suffers them to acquire, and which they abuse "*to the grievous hurt, damage, and destruction, of many of the king's liege people.*" It is often the consciousness of the part mankind take in their exertions, that stimulates the soldier in the field, and the orator in the council. Under the same powerful motive the physician would also perform wonders. But no such motive has ever operated upon medicine. The author once inscribed a book to the discoverer of a remedy which has rescued numbers from the tortures of the stone, as being in his opinion the member of society most deserving honor. The name, from a feeling that it ought long before to have been in every man's mouth, he omitted. Not less perhaps than a hundred well-informed persons have since asked him whom he meant. So true is it that few (according to the language of the old statute) "*discern the uncunning from the cunning*" in our art; and that in the great world, where celebrity and profit are chiefly to be acquired, there is much less chance of acquiring them by the most transcendent merit, than by a winning or imposing carriage.

By the indolent and the ill-informed I shall perhaps be told that our hospitals are already in able hands and that every thing useful to the present age and to posterity will be faithfully recorded: Consequently that no good can accrue from the cumbersome regulations I propose. But I, who could name a variety of hospitals, which in a long course of years have furnished nothing or next to nothing to medical philosophy, must be slow to believe in this universal vigilance. To many of our predecessors and many of our contemporaries the healing art is indeed deeply indebted for what they have recorded of their hospital practice. Nor would I desire a more conclusive argument. For if a part is so valuable, what would the whole be? And what, according to a just estimation, are the facts that have been preserved, but fragments of a mighty wreck, demonstrating the value of the mass that has perished?

The profession and the public being so far excited, the rest would easily follow. No remarkable phenomenon in private practice would remain unexamined and unauthenticated. Curiosity would have every where free scope: exercise would immensely improve the talent for observation; and no great improvement in the talent for observation can take place without a proportionate increase of success in practice.

Some readers may find it difficult to believe either that there is such a deficiency of physiological facts as I insinuate, or that an addition to the number would be cheaply purchased by any efforts and almost any sacrifice. But these sceptics should be reminded that science improves just as facts

multiply. The greater the number of phenomena known, the better is each understood. It will hardly be denied that books of medicine abound in falsified or erroneous or mixed narrations much more than in genuine facts. Their perplexed and absurd contents in general excite repugnance. By whom are they perused but by such as hope of profit or suffering impels to turn over a mountain of rubbish for the chance of picking up a grain of gold?—But such during its infancy is the chaotic state of the records of every science. Something is perceived, much imagined and little understood. Authors blend what they imagine with what they perceive; nor have they the prudence to refrain from attempting to explain what they do not understand. But in time they acquire patience to look more steadily and learn to distinguish better. Then the mists of imagination gradually disappear and objects are beheld in their natural shape and order. Thus exact information is procured; and it is among the precious advantages of exact information that, in adulterated narratives, it enables us to separate the false from the true, and prevents us from being deceived by wrong testimony. It would be impossible to impose a series of forged observations on the astronomers and chemists of Europe. And such as the spurious half-alchemical chemistry of the two last centuries is in our eye, such will our medicine appear when equal zeal shall have been kindled for physiological researches.

Nor is it necessary to appeal to the general analogy of science, to demonstrate our deplorable deficiency in physiological facts. It is impossible to think, or converse, or write concerning animal nature, without being stopped for

want of facts. Our latest systematic writer remarks that "as a great number of unconnected facts are difficult to be acquired, and to be reasoned from, the art of medicine is less efficacious under the direction of its wisest practitioners." Yet it is certain that to observe well and to connect well the phenomena occurring in a single case only, may be followed by the effect of saving many lives.

ANOTHER idea which I consider as of some importance long since presented itself to me. It has had a local circulation: and on the present occasion I shall lay it before the public at large. It bears some analogy to the preceding, inasmuch as it would render hospitals a greater source of improvement to medicine. Both ideas should be moulded into one system. But it is possible to realize one without the other; and I expect that if either is realized, it will be the latter.

My second proposal is *that the physicians and surgeons of hospitals be changed, or partly changed, every year, or every second year: that, if the average number of reputable physicians and surgeons residing in or near a place be sufficient for two changes, those who go out first be not re-elected till at least two periods shall have elapsed; and that the exclusion be prolonged according to the probability of a proper succession.*

I shall endeavour to shew that this plan is practicable, that it promises superior advantages in more than one respect, and that elections for an indefinite term are unjust towards the majority of the profession, and comparatively prejudicial to the public.

The rotation-scheme is practicable, for it is practised, at least without inconvenience, in more than one large city within the British dominions. This matter of fact will be proved, in the instance to which it refers, by the following letter from a physician, whose talents and respectability are sufficiently known. An extract would have been sufficient for my immediate purpose. But, as the letter describes certain humane arrangements that might be introduced elsewhere and is throughout interesting, I cannot prevail on myself to give it in a mutilated form.

Letter from R. CLEGHORN, M. D. Professor of Medicine in the University of Glasgow, to T. Beddoes, M. D.

Glasgow, July 15. 1797.

SIR,

It gives me great pleasure to hear that our infirmary has been honoured with the approbation of such a judge as Mr. WARR,* and I shall most willingly give you every information in my power concerning it.

Our printed papers are not worth sending. They refer merely to the admission of patients, the rights annexed to different subscriptions, and other things of that sort: however, if you shall still desire to see them, I will forward some copies by the first private opportunity.

Our infirmary owes much to its situation, which is high and open all around, with an elegant court before, and a

* I had mentioned my idea to Mr. W. who told me that my theory had been anticipated in practice, and referred me to Dr. Cleghorn.

large convenient garden behind. From a neighbouring canal, water rises between 30 and 40 feet above the foundation, and by forcing pumps, it is raised to the highest wards, each of which being supplied with a water closet, we are seldom troubled with such smells as you may remember on the side stairs of the Edinburgh infirmary. Our wards too are more convenient. There are no projections from the walls, and the bed-posts are all of iron. In short, every part is constructed, as much as possible, according to a report made to the late king of France, by a Committee of the academy, who were desired to point out the faults of the Parisian hospitals, and to specify the best mode of correcting them. Happening to get their report and plans, I gave them to Mr. Adams, who studied them carefully, and with much profit.

The management of our infirmary is vested in twenty-five members, of whom fifteen are official, (such as the member of parliament for the city, the provost (anglicé mayor) of Glasgow, the president of the faculty of physicians and surgeons, &c.) the other ten are elected annually from among the subscribers. The managers have stated quarterly meetings, at which they appoint committees, and examine every transaction connected with the business of the infirmary, and the result of all their proceedings, together with an account of the money received and expended, of the patients admitted and dismissed, &c. is laid every year before a general court of contributors, and afterwards published.

A committee of managers meet every friday in the infir-

mary, to examine the list of patients admitted or dismissed, and to take cognizance of any other material occurrences. Besides the managers, each of whom visits in his turn, twelve subscribers, not in the management, are named every year, as visitors for that year. The visitor goes daily, but at no stated hour, through every part of the house; he tastes the food, he examines the nurse respecting the behaviour of the patients, and the patients respecting the conduct of the nurse in the presence of each other. He remarks daily his report in a book kept on purpose, and each report is carefully investigated by the weekly committee who check the first appearance of oppression among the nurses, or of irregularity among the patients. This plan keeps all the servants on the alert from morning to night, and it is unspeakably gratifying to the poor patients to see so many interested in their welfare.

Two physicians are elected annually. Hitherto they have attended 3 months alternately; next winter they may probably attend both together for the convenience of the clinical students who have only an hour for the infirmary.

Four surgeons are elected annually. Two go out every year, and those who have attended two years, are not again eligible till after two years. The surgeons regulate their attendance by their own convenience; hitherto each has generally attended 3 months in turn.

A physician and surgeon attend every day at ten o'clock. Each examines his own patient, dictates reports, &c. just as in Edinburgh. They then visit the antichamber which is on the first floor, and there, besides examining the pa-

tients recommended for admission, they give advice to many others, sometimes thirty in a day. The attending physician and surgeon are officially members of the weekly committee, which they regularly attend along with their clerks to give information, especially concerning those patients who have been in the house above two months. If there be a prospect of relief, the patients are allowed to remain; if not, the recommender is obliged to remove them.

Two clerks live in the house. One is apothecary and surgeon's clerk, the other is physician's clerk. They write the cases and reports, visit the patients in the evening, &c. as in Edinburgh. They have bed, board, and washing, but no salary. No medical attendant has any salary.

Before any operation, both physicians, and all the surgeons are called to consultation. After examining the patient, and hearing the case, each gives his opinion (no student except the clerks being present) and the business is regulated by the opinion of a majority. We have a very convenient theatre in which the students see the operations very distinctly.

Our patients once amounted to ninety-four, which is the greatest number we ever had; last week they were seventy-three. When the house shall be completely furnished, it will be able to contain one hundred and forty-five, with their necessary attendants.

These are the chief things that occur to me. If you wish for any further information it will give me the utmost pleasure to communicate it. With great respect, I am,
Sir, your obedient servant,

ROBERT CLEGHORNE.

In the *clinical* wards of the Edinburgh infirmary, where accuracy is more especially necessary, since minute reports of the condition of all the patients are taken down each day, the physicians are changed twice in six months. And no essentially inherent inconvenience has ever been felt or imagined, in consequence of the transfer of the patients at the end only of a quarter of a year.

The case of Manchester, where I believe an entire change of the ordinary physicians and surgeons not long since took place, appears to me strictly in point. A regular succession has not, as far as I know, been established there. But on the resignation of the old set of practitioners, no disadvantage to the sick was complained of. Nor is it unlikely that this event, by awakening the minds of professional men and of others, contributed in some measure to the execution of a most successful plan for preventing the dissemination of febrile contagion, and generally promoted the purposes of the charity.

From the ascertained motives of human action, we may calculate that exclusive possession during life or pleasure, will have in part the baneful effects of monopoly. And that a perpetual quick succession of professional attendants will operate as a constant stimulus to exertion.

I am fully aware how liable such a remark is to invidious interpretation. I may be asked whether I mean to insinuate a general charge of negligence. But I am not speaking of culpable remissness. There is a wide difference between decent attendance, and that ardour which springs from emulation and the enjoyment of a fresh occupation

conjointly. It is the latter one would wish always to secure, when it can be had.

Having sometimes enquired, I have almost as constantly been informed, that *cæteris paribus*, the period immediately following their election, is that during which physicians and surgeons have most zealously interested themselves in the concerns of an hospital. It may indeed be expected that physicians and surgeons, in earnest about their profession and not yet fully employed, will regularly devote a large portion of time to hospital duty, whereas the fully employed can only make cursory and occasional visits. Is it not then to be expected, that a rotation can on no principle diminish the quantity of care bestowed upon the sick, and that it will probably on the whole much increase it?

In cities, and in the country, a considerable number of persons above the necessity of charitable aid, entrust their health to practitioners not belonging to any infirmary. Of this number it is the interest to have the advantages of medical information more equally diffused. And unvaried possession, when it does not absolutely cloy or disgust, is so well known to lower the tone of feeling, and impair the energy of exertion; that under a sense of rivalry occasional superintendence may be an equal, or a greater source of improvement.

The wrong done to the body of excluded practitioners (often men of the first talents in their respective professions) is much more flagrant than the disadvantage to the public, or to the charity. This is indeed a species of injury that ought never to be mentioned but in terms of the most pointed re-

probation, were it not certain that no system, capable of reconciling the interests of all parties, has generally been apprehended to be practicable.

When one asks—*why are votes ordinarily given for or against such a candidate?* there is danger of appearing personal. But certainly seldom in consequence of impartial comparison grounded on pure respect to merit, and the interests of the house. Chance has full dominion here. A man is born to such or such a connection. He is thrown among this or that set. So he gets his majority or minority. Reverse but this one circumstance, and the result of the election will generally be reversed. Few voters are capable of discriminating between a Sydenham, and a competitor whom no opportunities could ever enable to add an iota to the science of human nature. And some voters, though aware of the difference, would not have magnanimity to prefer the eternal benefactor of his species, to a countryman, a cousin, or a clubmate. Where therefore the votes stand as 400 to 40, the qualifications may very well be in the inverse proportion of the numbers. So perpetually do the causes of vogue in medicine remind the discerning spectator of Pope's question concerning another sort of favour!—

What shook the pit, and made the people stare?

It would therefore be idle to object that the circulating system may place the sick in inferiour hands. The system of perpetuity (for what any unprofessional person can tell) may keep them in inferiour hands. Nor is it meant that every unowned vagabond, styling himself doctor or surgeon,

shall be reputed to have an indefeasible title to an appointment at an infirmary. Subject the candidate to a reasonable probation; and then honestly allow fair presumptive claims, beyond which the judgment of a body of subscribers can never penetrate. Such claims are constituted by public services in the cause of a man's profession, by an adequate education, by a share of experience, by good sense and good conduct during a term of residence, long enough to afford some insight into character.

No equitable reader will construe an assertion of the just expectations of one set of men into disparagement of another set. It is only contended that if the office of public physician or surgeon be a burden, it ought to be equally borne—if an honour, it ought to be fairly shared. If there be practitioners out of infirmaries of as extensive reputation, as ardent in the cultivation of their departments, and to whom persons as high in rank, as dear to themselves, and as valuable to the world, entrust themselves, they are surely to be presumed not less qualified to take charge of the indigent sick.

There is reason for supposing that the suggested change would procure an occasional augmentation of the funds—an advantage not to be despised at a time when in some places distress has necessitated extraordinary measures, and obvious causes threaten defalcation of revenue. Individuals acquainted with medical men of unexceptionable character, of excellent general abilities, and unwearied attention to their profession, but less fortunate than some of their brethren, would be strongly induced to contribute to an institution, of

which *their* favourites may in turn hope to participate in the advantages, and to promote the purposes. The same motive may render the profession at large, more earnest in the exertion of their influence to procure subscriptions.

Were this proposal to engage the public attention, objections of detail would be urged by the interested, and perhaps by others. But such opposition every useful principle must expect to encounter. As many of the London hospitals have been converted into little schools of medicine, it may be said, that a reciprocation of physicians and surgeons would disturb the order of instruction. The inconvenience might be obviated, by allowing persons to lecture during the suspension of their other office. With this, most of the courses have no inseparable connection; and whatever is the present motive of these exertions, whether hope of profit or fame, it would remain. I rather think, however, that great good would arise, in different ways, from a succession of lecturers. The parties in expectation would take care to instruct themselves in many things, of which they are now content to remain ignorant or half-informed. By the efforts of various minds thus excited, many more combinations would be formed, than if such an incentive did not exist; and by these the public would inevitably profit, as well as the audience. That the talents of many great men are unproductive, for want of more diffused encouragement and opportunities, will be evident to those who are not weak enough to take it for granted that the most successful are *ipso facto* the most meritorious. To suppose that physicians who happen to be best known to a body of electors, are

precisely the men best qualified for an important medical station, derives from that credulity upon which was built the dogma of papal infallibility; and which in the present age, is more apparent in the sentiments of mankind respecting medicine than in those respecting religion. Had room been left for ingenious men to emerge, we might have seen during the same period, a dozen instead of a single Pott, and a single Hunter.

I doubt not but several of my readers have anticipated these arguments, and could furnish additional ones. To those of slower apprehension, I hope I have proved (or enabled them to prove to themselves) that a more liberal system would better secure both the primary and secondary ends of an infirmary: that by being frequently relieved each man would be more alert at his post; that no other plan can be deemed equitable towards the faculty at large; that so just a regulation while it quickened the diligence of some, would enlarge the experience of others: that, under its influence, these charities might be expected to furnish ampler supplies than they have yet done to the mass of medical knowledge: that by conceding equally to opinions, of which none can have any solid title to preference, it might bring more contributions to the general fund, exempt candidates from the humiliating preliminary of a canvass, mitigate electioneering animosities, and disentangle CHARITY from CABAL.

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* As Dr. Gibbes has related an experiment of his own on the Sodbury sulphate of strontian in *Mr. Nicholson's journal*, March 1799, without referring to any other person's previous observations, it seems but just to mention the following circumstances. In the collection of the Rev. Mr. Richardson at Bath, a friend of mine (Mr. Notcutt) instructed by Mr. W. Clayfield's specimens, pointed out some sulfate of strontian. Mr. Richardson gave a piece to Dr. Gibbes in January last. I had exhibited this substance, as found in other places near Bristol, to a large audience nine months before, viz. in spring 1798, and had sent specimens to Mr. W. Henry, who communicated the fact to the philosophical society at Manchester. Dr. Gibbes does not, undoubtedly, mean to claim the discovery. He knew last year that Mr. Clayfield was analysing the fossil. Hundreds of persons might have anticipated Mr. C. in announcing the fact. It is, indeed, I observe distinctly noticed in the *appendix to the monthly review*, vol. xxv. p. 580. June, 1798. The manner, in which the sulfate was detected in this neighbourhood, is exactly related below. EDITOR.

To Dr. BEDDOES,

AND

TO THE SUBSCRIBERS TO THE

PNEUMATIC INSTITUTION,

THESE ESSAYS ARE RESPECTFULLY

INSCRIBED.

AN ESSAY

ON

HEAT, LIGHT, and the Combinations of LIGHT.

A desire of improvement, and a peculiar spirit of philosophical investigation, productive of the greatest discoveries in the most important sciences, have eminently characterised this century.

Philosophers, not contented with examining the associations of those complex perceptions, to which the metaphysicians have given the name of abstract ideas, have observed nature, discovered effects, and erected their theories upon trains of connected sensations, called facts.

From hence arises the superiority of present philosophical systems ; though these systems are far from that perfection, which they appear capable of attaining by the new mode of investigation.

The sciences have not been equally improved ; those dependent for their existence on experiment, and the observation of physical phenomena, though newly discovered, have made the most rapid advances towards perfection.

Chemistry, which arose from the ruins of alchemy, to be bound in the fetters of phlogiston, has been liberated, and adorned with a beautiful philosophic theory. The numerous discoveries of Priestley, Black, Lavoisier, and the other European philosophers in this branch of science, afford splendid proofs of the increasing energies of the human mind.

From the application of Chemistry to the discovery of the laws of organic existence, mankind had hoped to derive the greatest advan-

tages ; from this source they expected the perfection of physiological science ; but their hopes have been in a great measure frustrated ; and if we except the theories of a celebrated medical philosopher, Dr. Beddoes, it will be found that chemistry has as yet afforded but little assistance in the cure of diseases, or in the explanation of the laws of organic existence.

Our ignorance of the composition of organic matter, and of the changes effected in the blood by oxygen gas, is a considerable source of the imperfection of medicine. A more intimate acquaintance with those important parts of chemistry would tend, not only to the elucidation of that branch of philosophy, but to the production of the most beneficial effects in medicine and physiology, sciences of the utmost importance to man ; sciences, from the perfection of which he may hope to eradicate a great portion of the physical and moral evil to which he is subject.

LIGHT has been heretofore little considered in chemical theory ; its affinities have never been investigated. A substance of the greatest importance to organic existence has been very little regarded, but in a physical view, as a stimulus, and as the source of the most numerous and pleasurable of our sensations.

The planetary motions, those wonderful phenomena, and the laws by which they are governed, appear to be designed for the express purpose of supplying the whole of the solar system with a certain necessary quantity of light.

The general analogy of nature, the wonderful simplicity of causes and complexity of effects, would alone tend to prove that this substance is subservient to other purposes than those of vision and vegetation. Since light and heat are usually concomitant, since there is rarely a considerable degree of one without the other, philosophers have questioned whether they are not cause and effect ; and M. Lavoisier is one

of these philosophers. He says:* “ La lumière, est elle une modification du calorique, ou bien le calorique, est il une modification de la lumière ? ” I have made an experiment which seems to demonstrate directly that light is not a modification, or an effect of heat.

EXPERIMENT I.

A small gunlock was procured, armed with an excellent flint. This lock was elevated by means of two iron springs on the stand of the receiver of an air-pump. A flight iron wire was affixed to the trigger, brought through a hole made in the centre of the stand, and cemented into the hole with wax, so as to exclude entirely atmospheric air from the receiver. The receiver was exhausted, and the lock snapped; but no light was produced. The receiver was filled with carbonic acid, and the lock again snapped, with the same result; no light was produced. Small particles were separated from the steel, which on microscopic

* *Traité élémentaire.* t. 1. p. 6.

examination evidently appeared to have undergone fusion.

If light was a modification, or an effect of heat, it must have been produced in this experiment; since the heat generated by collision was sufficient to fuse steel, a degree of heat much above that improperly called a white heat.

Light then cannot be caloric in a state of projection.

Nor can it be, as some philosophers suppose, a vibration of the imaginary fluid ether. For even granting the existence of this fluid, it must be present in the exhausted receiver, and in carbonic acid gas, as well as in atmospheric air; and if light is a vibration of this fluid, generated by collision between flint and steel in atmospheric air, it should likewise be produced in the exhausted receiver, where a greater quantity of ether is present, which is not the case.

Since light is neither an effect of caloric, nor of an ethereal fluid supposed to be extended through space,* and as the impulse of a material body on the organ of vision is essential to the generation of a sensation, light is consequently matter of a peculiar kind, capable when moving through space with the greatest velocity, of becoming the source of a numerous class of our sensations.

Matter is possessed of the power of attraction. By this power the particles of bodies tend to approximate, and to exist in a state of contiguity. The particles of all bodies with which we are acquainted, can be made to approach nearer to

* The philosophers who support this opinion suppose the universe a plenum; amongst these is the great Euler. It may not be amiss to observe on this subject, that to suppose the universe a plenum, and constituted a plenum by an elastic fluid, is absurd. It is essential to the elasticity of a fluid, that it be capable of compression; that is, of filling less space than it before existed in. Now as different bodies cannot exist at the same time in the same place, there must necessarily be a void space between the particles of an elastic fluid, such as ether is assumed to be, which overturns their hypothesis.

each other, by peculiar means, that is, the specific gravity of all bodies can be increased by diminishing their temperatures. Consequently (on the supposition of the impenetrability of matter) the particles of bodies are not in actual contact. There must then act on the corpuscles of bodies some other power, which prevents their actual contact ; this may be called repulsion. The phænomena of repulsion have been supposed, by the greater part of chemical philosophers, to depend on a peculiar elastic fluid ; to which the names of latent heat, and caloric, have been given. The peculiar modes of existence of bodies, solidity, fluidity, and gazity, depend (according to the calorists) on the quantity of the fluid of heat entering into their composition ; this substance insinuating itself between their corpuscles, separating them from each other, and preventing their actual contact, is, by them, supposed to be the cause of repulsion.

Other philosophers, dissatisfied with the evidences produced in favour of the existence of

this fluid, and perceiving the generation of heat by friction and percussion, have supposed it to be motion.*

Considering the discovery of the true cause of the repulsive power as highly important to philosophy, I have endeavoured to investigate this part of chemical science by experiments: from these experiments (of which I am now about to give a detail,) I conclude that heat, or the power of repulsion, is not matter.

* It was foreign to the design of this essay to give the history of the opinions of the different philosophers on the cause of heat, as well as an account of the different experiments that have heretofore been made on it. The short view of the phænomena of repulsion given after the experiments on the cause of heat, is deduced from the experiments of Black, Crawford and others, experiments which are too well known by chemical philosophers to need quotation.

The phænomena of repulsion are not dependant on a peculiar elastic fluid for their existence, or Caloric does not exist.

Without considering the effects of the repulsive power on bodies, or endeavouring to prove from these effects that it is motion, I shall attempt to demonstrate by experiments that it is not matter; and in doing this, I shall use the method called by mathematicians, *reductio ad absurdum*.

Let heat be considered as matter, and let it be granted that the temperature of bodies cannot be increased, unless their capacities are diminished from some cause, or heat added to them from some bodies in contact.

Now the temperatures of bodies are uniformly raised by friction and percussion. And since an increase of temperature is consequent on friction and percussion, it must consequently be generated in one of these modes.

First, either from a diminution of the capacities of the acting bodies from some change induced in them by friction, a change producing in them an increase of temperature.

Secondly, or from heat communicated, from the decomposition of the oxygen gas in contact by one or both of the bodies, and then friction must effect some change in them (similar to an increase of temperature) enabling them to decompose oxygen gas, and they must be found after friction, partially or wholly oxydated.

Thirdly, or from a communication of caloric from the bodies in contact, produced by a change induced by friction in the acting bodies, enabling them to attract caloric from the surrounding bodies.

Now first let the increase of temperature produced by friction and percussion be supposed to arise from a diminution of the capacities of the acting bodies. In this case it is evident some change must be induced in the bodies by

the action, which lessens their capacities and increases their temperatures.

EXPERIMENT II.

I procured two parallelopipedons of ice,* of the temperature of 29° , six inches long, two wide, and two thirds of an inch thick: they were fastened by wires to two bars of iron. By a peculiar mechanism, their surfaces were placed in contact, and kept in a continued and violent friction for some minutes. They were almost entirely converted into water, which water was collected, and its temperature ascertained to be 35° , after remaining in an atmosphere of a lower temperature for some minutes. The fusion took place only at the plane of contact of the two pieces of ice, and no bodies were in friction but ice. From this experiment it is evident that ice by friction is converted into

* The result of the experiment is the same, if wax, tallow, resin, or any substance fusible at a low temperature be used, even iron may be fused by collision, as is evident from the first experiment.

water, and according to the supposition its capacity is diminished ; but it is a well known fact, that the capacity of water for heat is much greater than that of ice ; and ice must have an absolute quantity of heat added to it, before it can be converted into water. Friction consequently does not diminish the capacities of bodies for heat.

From this experiment it is likewise evident, that the increase of temperature consequent on friction cannot arise from the decomposition of the oxygen gas in contact, for ice has no attraction for oxygen. Since the increase of temperature consequent on friction cannot arise from the diminution of capacity, or oxydation of the acting bodies, the only remaining supposition is, that it arises from an absolute quantity of heat added to them, which heat must be attracted from the bodies in contact. Then friction must induce some change in bodies, enabling them to attract heat from the bodies in contact.

EXPERIMENT III.

I procured a piece of clock-work so constructed as to be set to work in the exhausted receiver; one of the external wheels of this machine came in contact with a thin metallic plate. A considerable degree of sensible heat was produced by friction between the wheel and plate when the machine worked uninsulated from bodies capable of communicating heat. I next procured a small piece of ice;* round the superior edge of this a small canal was made and filled with water. The machine was placed

* The temperature of the ice and of surrounding atmosphere at the commencement of the experiment was 32° , that of the machine was likewise 32° . At the end of the experiment the temperature of the coldest part of the machine was near 33° , that of the ice and surrounding atmosphere the same as at the commencement of the experiment; so that the heat produced by the friction of the different parts of the machine was sufficient to raise the temperature of near half a pound of metal at least one degree, and to convert 18 grains of wax (the quantity employed) into a fluid.

on the ice, but not in contact with the water. Thus disposed the whole was placed under the receiver (which had been previously filled with carbonic acid), a quantity of potash (i. e. caustic vegetable alkali) being at the same time introduced.

The receiver was now exhausted. From the exhaustion, and from the attraction of the carbonic acid gas by the potash, a vacuum nearly perfect was, I believe, made.

The machine was now set to work. The wax rapidly melting proved the increase of temperature.

Caloric then was collected by friction ; which caloric, on the supposition, was communicated by the bodies in contact with the machine. In this experiment, ice was the only body in contact with the machine. Had this ice given out caloric, the water on the top of it must have been frozen. The water on the top of it was not frozen, consequently the ice did not give

out caloric. The caloric could not come from the bodies in contact with the ice ; for it must have passed through the ice to penetrate the machine, and an addition of caloric to the ice would have converted it into water.

Heat, when produced by friction, cannot be collected from the bodies in contact, and it was proved by the second experiment, that the increase of temperature consequent on friction cannot arise from diminution of capacity, or from oxydation. But if it be considered as matter, it must be produced in one of these modes. Since (as is demonstrated by these experiments) it is produced in neither of these modes, it cannot be considered as matter. It has then been experimentally demonstrated that caloric, or the matter of heat, does not exist.

Solids, by long and violent friction, become expanded,* and if of a higher temperature than

* Expansion by friction is common to almost all bodies ; and as the exceptions are very few, it may be admitted as

our bodies, affect the sensory organs with the peculiar sensation known by the common name of heat.

Since bodies become expanded by friction, it is evident, that their corpuscles must move or separate from each other. Now a motion or vibration of the corpuscles of bodies must be necessarily generated by friction and percussion. Therefore we may reasonably conclude that this motion or vibration is heat, or the repulsive power.

Heat then, or that power which prevents the actual contact of the corpuscles of bodies, and which is the cause of our peculiar sensations of heat and cold, may be defined a peculiar motion, probably a vibration, of the corpuscles of

a principle. I have found by experiment, that the metallic substances, and the solid combinations of hydrogen, carbon, and oxygen, become enlarged in all their dimensions when heated by friction, and I believe all other bodies except ice, in which a new apposition of particles, and probably a new repulsive motion takes place.

bodies, tending to separate them. It may with propriety be called the repulsive motion.*

Since there exists a repulsive motion, the particles of bodies may be considered as acted on by two opposing forces, the approximating power, which may (for greater ease of expression) be called attraction, and the repulsive motion. The first of these is the compound effect of the attraction of cohesion, by which

* Heat, in common language, signifies that sensation which accompanies an increase of repulsive motion in any part of our system. It should not be used therefore for the repulsive motion or cause of that sensation.

The caloric of the French nomenclators is equally exceptionable; for having been generally used to express the imaginary fluid, or matter of heat, it is now associated with, and generally suggests that idea, and would thus if used to express the repulsive motion, or cause of heat, become a source of error. Words expressing compound ideas, should, when formed at will, express as near as possible the component parts of these ideas, when they are known. The word repulsive motion is, I believe, liable to no exception, I shall therefore use it to express the separating power of the corpuscles of bodies, and the cause of our sensation of heat.

the particles tend to come in contact with each other, the attraction of gravitation, by which they tend to approximate to the great contiguous masses of matter, and the pressure under which they exist, dependant on the gravitation of the superincumbent bodies. The second is the effect of a peculiar motory or vibratory impulse given to them, tending to remove them farther from each other, and which can be generated, or rather increased, by friction or percussion. The effects of the attraction of cohesion, the great approximating cause, on the corpuscles of bodies, is exactly similar to that of the attraction of gravitation on the great masses of matter composing the universe, and the repulsive motion is analagous to the planetary projectile force.

Bodies exist in different states, and these states depend on the differences of the action of attraction, and of the repulsive power, on their corpuscles, or in other words, on their different quantities of attraction and repulsion.

When the attraction predominates over the repulsive motion, the body exists in the state of solidity. In this state its particles are relatively contiguous, consequently it exists in a relatively small space. It is difficultly divisible by mechanical means; and when divided, its parts being brought into apparent contact, are incapable of aggregative union.

When the sum of the attractions of all the corpuscles is nearly equal to that of their repulsive motion, that is, when the attraction and repulsion are in equilibrio, the body exists in the state of fluidity. In this state it is difficultly compressible, and easily divisible by mechanical means, and when divided, its parts being brought into apparent contact, are capable of aggregate union.

When the repulsive motion predominates over the attraction, the body exists in the state of *gazity*, or elastic fluidity. Existing in this mode, it fills up great space; it is more easily divisible, and more capable of aggregative

union than a fluid, and has the peculiar property of elasticity, that is, of great diminution of volume by compression, and of expansion by abstraction of compressing forces ; so that its volume is in the inverse ratio of the compressing weights.

There is another state which has been heretofore unnoticed, and in which only one body that we are acquainted with exists, namely light.*

In this state the repulsive motion predominates to such an extent over the attraction, that the corpuscles indefinitely separate with the greatest velocity, and appear to be very little acted on by attraction or gravitation. This state may be called repulsive projection.†

* And probably odorous matters, as they appear to be continually flying off with great velocity from bodies.

† As this a state of existence hitherto not particularly noticed, it was thought proper to distinguish it from other

Every body with which we are acquainted exists in one of these states, and appears capable of existing in either of them by the increase or diminution of the repulsive motion of its corpuscles. But though these are modes of existence common to matter in general, yet it appears that all bodies have different quantities of attraction and repulsive motion when existing in the same state. Different solids, fluids, and gasses are alike eminently different in their specific gravity, and this difference would induce us to suppose that the sums of the attractions and repulsive motions of their particles are different.

When bodies expand, we are certain that their repulsive motion is increased; when they contract, we are certain it is diminished; and

states by a peculiar name. In this state the repulsive motion predominates to such an extent over the attraction, that the particles are projected into space with the greatest velocity. The term repulsive projection will distinguish this state from others with sufficient accuracy, and from mechanical projection.

we have no other infallible tests of an increase or diminution of repulsive motion, but expansion or contraction.

Bodies may have their repulsive motion increased in three modes.

First, By the transmutation of mechanical into repulsive motion, that is, by friction or percussión. In this case the mechanical motion lost by the masses of matter in friction is the repulsive motion gained by their corpuscles.

Secondly, By the motion of chemical combinations or decomposition.

Thirdly, From the communicated repulsive motion of bodies in apparent contact.

The disposition in bodies to communicate or receive the repulsive motion, has been called temperature, and the temperature of a body is said to be high or low in proportion as it com-

municates or receives the repulsive motion. The powers to communicate or receive the repulsive motion, as well as the velocities of communication or reception, are specifically different in different bodies, and as far as we know, do not depend on the absolute quantities of repulsive motion, but on some peculiar atomic constitution now unknown to us. As it is found by experiment that the disposition of almost all bodies to communicate repulsive motion is increased by every addition of it as measured by expansion, and diminished by every subtraction as measured by contraction, the temperatures of bodies are generally made the measures of their relative quantities of repulsive motion.*

* A peculiar sensation known by the name of heat is consequent on an increase of repulsive motion in any part of our bodies, and an opposite one called cold on a diminution of it. The common mode of determining the relative quantities of repulsive motion in bodies, is by appealing to these sensations. Philosophy uses the mercurial thermometer. The discovery of any mode of accurately determining the contractions and expansions of solids, would be a great acquisition to science.

Different bodies have their temperatures, or their powers to communicate or receive repulsive motion differently increased by the addition, and diminished by the subtraction, of equal quantities of repulsive motion. This disposition is called, in the doctrine of caloric, their capacity for heat; but it might be named with greater propriety, their capability of temperature, for it has no relation to the absolute quantity of repulsive motion they are capable of receiving, but only to their temperature. All bodies, in fact, are capable of any increase of repulsive motion, but have their temperatures differently raised by this increase, that is, have different capabilities of temperature. The body then, that is said to have the greatest capacity for heat, has the least capability of temperature, and vice versa. The capability of temperature of bodies is diminished by the addition of repulsive motion, and increased by its subtraction; so that the capability of a body is greatest when solid, less when fluid, and least of all when in the gaseous, or repulsive projectile state.

The capability of temperature of bodies likewise depends on the degree of pressure under which they exist. When bodies are compressed, their capabilities are increased, and their temperatures raised ; when pressure is removed from bodies, their capabilities are diminished, and their temperatures lowered.

When bodies in apparent contact communicate the repulsive motion, the motion gained or lost by one body is uniformly equal to that lost or gained by the other body, as measured by temperature.

When two similar and equal bodies are brought in contact, they acquire a common temperature by communication of their repulsive motion : and the common temperature is an arithmetical mean between the two original temperatures. When two unequal and similar bodies are brought in contact, if their temperatures are different, they will acquire a common temperature by communication, and the communicated repulsive motion will be consequently

divided between them in proportion to their quantities of matter. If two bodies of different capabilities of temperature, and of different temperatures, be brought in contact, they will acquire a common temperature, and the communicated repulsive motion will be found divided between them in proportion to their quantities of matter, and their capabilities.

When bodies combine chemically, it generally happens that the capability of the compound is different from that of either of the constituents. Hence in chemical combinations, the capabilities of bodies are either increased or diminished. When the capabilities of bodies are increased by chemical combination, the temperature of the compound is greater than that of the constituents. When the capability of the compound is less than that of the constituents, its temperature is diminished.

To ascertain the causes of the increase or diminution of temperature consequent on chemical combination, is by far the most difficult part of

the philosophy of heat. For we are hardly able to distinguish the increase of temperature in bodies generated by the motion of combination, from that generated by increase of capability, and the motion of combination interferes with the diminution of temperature, from diminution of capability ; and chemical processes are in general so complex, that we cannot distinguish between the increase of repulsive motion from composition, and that arising from decomposition.

We have every reason to suppose from the foregoing experiments, and observations that the repulsive power of bodies is a peculiar motory impulse. To distinguish this motion from others, and to signify the cause of our sensations of heat, &c. the name *repulsive motion* has been adopted.

On the theory of caloric, a peculiar elastic fluid was supposed to exist between the particles of all bodies ; and the peculiar modes of existence of bodies, that is, solidity, fluidity, and

gazity, were supposed to depend on the different quantities of caloric entering into their composition. The elasticity, that is, the compressibility of this fluid, has been universally admitted, and the compressibility of the gases has been supposed to depend on the compressibility of their caloric.

Now since Caloric is supposed compressible, that is, capable of having its volume diminished by pressure, its particles cannot be in actual contact; there must consequently act on them some power which prevents their actual contact, that is, the repulsive motion. So that to admit the existence of an imaginary fluid in conformity to the absurd axiom, *bodies cannot act where they are not*, is in fact the solution of a small difficulty by the creation of a great one. After all, a principle must be admitted, (that is, repulsion ;) to do away the necessity of which, caloric has been invented.

On the theory of repulsive motion, it is evident that the gases (which M. Lavoisier and

the French nomenclators have assumed to be simple substances combined with caloric) must be either simple substances in the state of elastic fluidity, or combinations of two or more simple substances. Hydrogen and nitrogen gas we have not yet been able to decompose; they are then relative to the present state of our knowledge, simple substances. Oxygen gas, as will be hereafter proved, is composed of light and oxygen.

Since the word gas adopted by the French nomenclators is intended to express the chemical combination, or rather the saturation of bodies with caloric, it is doubtless exceptionable. The following arguments will, I think, prove that it ought not to remain in the chemical nomenclature.

For first, Bodies, when rendered from solids to fluids, from fluids to gases, are not essentially altered, their corpuscles are farther separated, that is, they move in greater space than before; but the body is equally simple, it has under-

gone no decomposition or combination; the word then that expresses the chemical combination of bodies with caloric is improper.

Secondly, All bodies with which we are acquainted have certain quantities of repulsive motion; and they have different modes of existence dependent on their quantities of repulsive motion. The most common of these modes are solidity, fluidity, and gazity; in each of these states the bodies are equally simple. That state in which they are found at the common temperature of the atmosphere, is the state from which they derive both their common and philosophic names. Now the French nomenclators have called all simple substances (the gases excepted) by their common names, without making any alteration to express their combination with caloric. They have called all the metals, which are capable of existing like other bodies, in three states, by their common names; and on their own principles these bodies are combined with caloric. They should therefore have distinguished them by names

expressing this combination, and have called gold, mercury, and sulphur, solid gold, fluid mercury, and solid sulphur, for the same reasons that they have given the names of hydrogen and azotic gas to the mephitic and inflammable airs, which are probably metals in the state of elastic vapor.

Thirdly, Those substances which have been called gases, uniformly exist in the state of elastic fluidity, at the common temperature of our planet. Simple names, without the addition of gas, would distinguish them from all other substances. With the same propriety that we use the term hydrogen gas in chemistry, we might make use of the terms, solid gold, fluid mercury, and fluid alcohol. In treating of the changes made in bodies by the repulsive motion, we may with propriety use the terms, solid, fluid, and gaseous, to express the different modes of existence of the same body. But simple substances should be distinguished by names characteristic of their properties; compound substances should be distinguished by names

expressive of the combination, that is, of the substances forming the compound. In conformity to these principles, I shall omit any names signifying the peculiar modes of existence of bodies; treating of substances, I shall give them their simple names, and by these names I mean to express the state in which they exist at the common temperature of the atmosphere. As for example, in using the words gold, mercury, and hydrogen, I mean solid gold, fluid mercury, and gaseous hydrogen. Oxygen gas, (which the French nomenclators have assumed to be oxygen combined with caloric) will be proved to be a substance compounded of light and oxygen. It would be highly improper to denote this substance by either of the terms oxygen gas, or oxygen. The one would signify that it was a simple substance combined with caloric, the other that it was a simple substance, the acidifying principle. The term *phos oxygen* (from $\phi\omega\varsigma$ light, $\alpha\epsilon\upsilon\varsigma$ acid, and $\gamma\epsilon\gamma\eta\tau\omega\rho$ generator) will I think be unexceptionable; it will express a chemical combination of the simple substance light, with

the simple substance oxygen ; it will not materially alter the nomenclature of the French philosophers ; and as will be seen hereafter, it can be easily modified to express, in conjunction with other words, the combinations of light and oxygen.

Of LIGHT.

Light is a body in a peculiar state of existence. Its particles are so amazingly minute, that they are very little affected by gravitation ; and pass unaltered through the pores of diaphanous bodies. They move through space with a velocity almost inconceivable, and communicate no perceptible mechanical motion to the smallest perceptible particles of matter.* From the peculiar velocity of light we estimate its quantity of repulsive motion. The influence of the attraction of gravitation on light is very small, as is evident from its not apparently gravitating towards the sun or the earth. The influence of the cohesive attraction on its particles is likewise very small, as is evident from their uniform separation, &c. ; but the repulsive motion acting on the corpuscles of light is very great, as is apparent from their

* It will be by and by proved that they communicate portions of their repulsive motion to the corpuscles of bodies.

velocities, and continual separating motion. But as we have said before, the distances of the corpuscles of bodies from each other, and the velocities of their motions, are in a ratio compounded of their repulsive motion and attraction. When the repulsive motion eminently predominates over the cohesive and gravitative attraction, the particles of matter will indefinitely separate, as those of light. To distinguish this state of existence, peculiar to light, from those other bodies, we have given it the name of *repulsive projection*.

Light is the source of the most numerous and pleasurable of our perceptions. This tribe of perceptions is thus received; particles of light in the state of repulsive projection coming in contact with the retina, communicate to it portions of their repulsive motion. The retina appears to be composed of nervous medulla and as some suppose of irritable fibre. The communicated motion of light either stimulates the irritable fibre into contraction, which contraction is accompanied with that affection of the nerve correspond-

ing to a sensation : or the motion of light communicated to the nerve itself produces the sensorial affection. The former of these opinions is rendered probable by the experiments of Dr. Darwin on ocular spectra.* It is then necessary to our perceptions of light, that it exist in its peculiar state of repulsive projection ; we consequently cannot perceive it by vision in any other mode of existence.

It appears from experiment that our sensations of vision are occasioned by the united impulse of a number of particles of light both synchronically and successively falling on the retina. All our different sensations then must arise from differences in the particles of light, their motions and numbers, or from differences in some of these.

Light passing through diaphanous bodies, is attracted by their particles, and the attraction of the particles of bodies for light is proportional

* See the end of Darwin's *Zoonomia*, Vol. 1.

to their density and combustibility. The laws of the attraction of diaphanous bodies for light are the laws of refraction which have been so admirably explained by the immortal Newton. He discovered that the particles of solar light are not equally attracted in passing through different bodies: they are differently refracted in passing through the prism, and separated into seven classes of particles that produce the sensations of red, orange, yellow, green, blue, indigo, violet. This difference is easily accounted for, by supposing that the particles of light in taking the state of repulsive projection, originally received a different repulsive motion. The red particles being supposed to vibrate with the greatest velocity, must be least attracted by the particles of the medium through which they pass; the violet particles moving with the least velocity, must be most refracted, and the different velocities of the intermediate coloring particles being supposed, their different refractions must be correspondent, which is found to be the case.

Light is reflected from bodies that it cannot penetrate or combine with. The sensations we receive from reflected light are eminently different. A correspondence has been long observed between the colors of bodies, and the increase of repulsive motion in them from the action of light. This correspondence is a subject of great importance to physical science, and worthy of investigation. Heat has been proved to be a peculiar repulsive motion of the particles of bodies. Light is a body, the particles of which are acted on by the greatest repulsive motion. When a body has its repulsive motion increased by the action of light, a portion of the repulsive motion of light must be lost, a portion equal to that gained by the body acted on ; this is evident from what has been said on the communication of repulsive motion in the doctrine of heat.

To determine accurately the correspondence between the increase of repulsive motion in bodies from the action of light, and their colors, I made the following experiment.

EXPERIMENT IV.

Six similar pieces of copper,* of equal weight, size, and density, were thus colored, one white, one yellow, one red, one green, one blue, and one black. A portion of a mixture of oil and wax, which became fluid at about 76° , was placed on the centre of each on the inferior side. They were then attached to a board painted white, and so placed with regard to the sun, that their upper surfaces were equally exposed to the light. Their inferior surfaces, to which the cerate was attached, were equally deprived of light and heat, that is, they were so exposed, that there could be no mistake with regard to the repulsive motion generated in them by the action of light. The changes of temperature in them from the action of light, took place in the following order. The cerate on the black plate began to melt perceptibly before the rest, the blue next in order, then the green and the red, and lastly the yellow ;

* Each a cubic inch square, and two lines thick.

the white was scarcely at all affected when the black was in a complete state of fusion.

This experiment proves that the increase of repulsive motion in bodies from the action of light, is great in proportion as the colors are dark. Now as our sensations arise from the united impulse of a number of particles on the retina, in proportion as the vibratory motions of these particles are greater or less, so in proportion must our sensations be different.

It does not appear that reflected light is in any instance composed of particles that have equal quantities of repulsive motion ; the differences of sensations therefore most probably arise from the differences occasioned in the vibrations of all the particles, from their communicating to the reflecting bodies portions of their repulsive motion, and not from the communication of the greater portion of that of some of the particles, that is, of those which are supposed in the Newtonian system to be absorbed. On this supposition the light reflected from white bodies

(which may be said to have the least capability of subtracting the repulsive motion of light) must vibrate with the greatest velocity, that is, must be reflected nearly unaltered. The particles reflected from dark bodies (which may be said to have the greatest capability for receiving the repulsive motion of light) communicating great portions of their repulsive motion, must vibrate with the least velocity, and all the intermediate colors may depend on the different velocities of vibration. Their vibrations, though of different lengths, may be isochronous, and all the particles may pass through rectilinear space in equal times.

Bodies perfectly black must subtract so much of the repulsive motion of light, as to deprive it of its repulsive projectile form. The electric fluid is probably light in a condensed state, that is, not supplied with the repulsive motion sufficient to give it repulsive projection. Its chemical action upon bodies is similar to that of light, and when supplied with repulsive motion by friction, or the contact of bodies from which

it is capable of subtracting it, it takes the repulsive projectile form, and becomes perceptible as light. It is extremely probable that the great quantity of this fluid almost every where diffused on our earth, is produced from the condensation of light, from the subtraction of its repulsive motion by black and dark bodies. This fluid continually formed from the condensation of light, is probably again supplied with repulsive motion at the poles, by the revolution of the earth on its axis, and given off in the form of repulsive projectile light, whilst a quantity equal to that given off from its equilibrating principle is supplied continually from the other parts of the globe. Hence the phænomenon of the aurora borealis, or northern lights. No more sublime idea can be formed of the motions of matter, than to conceive that the different species are continually changing into each other. The gravitative, the mechanical, and the repulsive motions, appear to be continually mutually producing each other, and from these changes all the phænomena of the mutation of matter probably arise.

Though the temperature of the atmosphere, which is found proportional to the light passing through it, may in some measure depend on the repulsive motion communicated to it by opaque colored bodies ; yet it is reasonable to suppose that the particles of light in passing through the atmosphere, lose small portions of their repulsive motion, which is the great cause of the atmospheric heat. Water, glass, and other transparent bodies, are capable of having their repulsive motion increased by the action of light ; and light in passing through them becomes colored independent of decomposition. A body perfectly white appears colored in deep water, and Dr. Halley, when in the diving bell, found that his hand, exposed to the solar light, became of a deep red. Is not the blue color of the air a proof that the repulsive motion of light is diminished in passing through it ? May not the atmospheric temperature, and the refraction of light in it be in a great measure owing to the water held in solution by the air, for the temperature is lower in proportion as we advance higher in the atmosphere, and refraction does

not take place above forty-five miles high, and we are certain from the phænomena of fiery meteors, that the atmosphere extends at least as high again.

On the above mentioned supposition, all our infinitely different sensations from reflected light must arise from differences in the numbers and repulsive velocities of the particles, and all light, if we may appeal to our sensations, must be in some measure altered both by reflection and refraction.

Bodies that do not contain light in combination, and that are incombustible, under certain circumstances become luminous.

To discover the cause of the luminous appearance of incombustible bodies, it is necessary to consider the circumstances under which it takes place. When glass, flint, argil, a metallic oxyd, or any other incombustible body, is exposed to a strong light, as the focus of a lens, its temperature is gradually raised, that

is, its repulsive motion is increased. After a certain time it become luminous, white, or red hot : and if it be now removed out of the focus, it continues for some time to give out light, and to communicate repulsive motion to the surrounding bodies, till the equilibrium of temperature is restored.

Now when bodies have their repulsive motion increased by the action of light, it is evident that the motion gained by the body, is that lost by the light ; and as a certain quantity of repulsive motion is essential to its repulsive projection, it must after considerable communication of repulsive motion, cease to exist in that state. In its new state of existence it is probably not perceptible to any of our senses. We therefore in this case can only reason from its effects. The body continues for some time to give off light, after being removed out of the focus ; light therefore must have been condensed in some form around it, and being gradually supplied with repulsive motion from the body, flies off slowly in the repulsive pro-

jeſtile form, becomes perceptible as light, and is the cauſe of the peculiar ſenſation known by the name of red or white heat.*

From what has been ſaid, it is evident that light, both in the ſtate of repulſive projection,

* This theory of the luminous appearance of incombustible bodies may be compared with Macquer's and Newton's.. Fourcroy ſays, “ L'incandescence des corps incombustibles, telles que les pierres dans lesquelles on ne peut point admettre la presence de la lumiere combinée, au moins comme dans les corps incombustibles, a été expliquée d'une maniere tres ingenieuse par Macquer. Suivant ce chimiste, elle depend des vibrations fortes, excitées. dans les molecules de ces corps par la chaleur ; ces vibrations dispose les particles de sorte que leur facettes, sans cesse agitées sont autant de petit miroirs qui reflechissent vers nos yeux les rayons de lumiere, qui existent dans l'air pendant la nuit autant que pendant le jour, et qui ne sont insensibles, et ne produisent les tenebres que parceque leur direction ne se fait pas sur les organes de la vue.

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The immortal Newton ſays, “ Do not all bodies when heated beyond a certain degree, emit light and ſhine, and is not this emission performed by the vibratory motions of their parts,” &c. See 8, 9, and 10 queries at the end of his optics. The firſt experiment appears to overturn the foundation of theſe theories.

and in the form of the electric fluid, performs an important part in the physical phenomena of the universe. In any other states than these, we have not hitherto been able to detect it by the senses ; but we know not what we may be enabled to accomplish by means of a more extensive and philosophic chemistry. A number of the elements are already in our power, and the rapid progress of the perfection of science seems to promise us the knowledge of those etherial fluids, which at present elude the perception of our organs, and are only known by their effects.

The chemical effects of light are not less important than the physical. Its combinations, hitherto almost unnoticed, have the highest connection (as will be seen hereafter) with organic existence ; and the most astonishing and beautiful of the chemical phenomena depend upon them.

Light enters into the composition of a number of substances. In some of these, the incombuſtible phosphorescent bodies, it most probably

exists in a state of loose combination, and the presence or absence of light does not appear to alter their properties materially. In *phosphorygen* (oxygen gas) it is intimately combined with oxygen.

Of the COMBINATIONS of LIGHT.

Of PHOSPHORESCENT BODIES.

Certain bodies, after being for some time exposed at a high temperature to light, continue luminous for a considerable length of time after this exposure. Such are many preparations of lime, the bolognian stone, &c. This phænomenon is in some measure analogous to the ignition of incombustible bodies.

Light, it appears, is only susceptible of combining, and of remaining in combination with those bodies at a higher temperature than that of our atmosphere ; at the common temperature it is liberated.

The name of solar phosphori has been given to

these bodies, in common with all others that become luminous, independant of combustion.

Other bodies exist, which become luminous when their repulsive motion is increased by communication of it from some bodies of a higher temperature. Light remains in combination with these bodies only at a low temperature. When their repulsive motion is increased, the light is liberated. This decomposition appears to arise from the diminution of the chemical attraction between light and the body, by the repulsive motion, and from the supply of a quantity of it sufficient to enable light to fly off in the repulsive projectile form. Amongst these bodies are the different combinations of lime and particularly the fluates (the colors of which appear to depend upon combined light), different combinations of barytes, the sulphate of potash, some of the metallic oxyds, cotton, wool, oils, wax, alcohol, &c. We owe the discovery of the greater number of these phosphorescent bodies to Mr. T. Wedgwood.*

* See his ingenious paper in the phil. trans. for 1792.

We have found that the sulphate of strontian, on an increase of temperature, gives out a pale light.

There is a class of phosphorescent bodies, which give out their combined light on attrition. Amongst these are borate of soda, sulphate of argil, tartrate of potash, and all the silicious class of stones. § This phosphorescence may be accounted for in the same manner as the last species.

Certain substances give out their combined light on immersion into the mineral acids. When magnesia* is thrown into the sulphuric acid, a light is liberated which produces a sensation similar to that known by the name of red heat. The same effect is produced when the nitric acid is used.

§ For a further account of these bodies, see the same paper of Mr. T. Wedgwood.

* I was informed of this phenomenon by Dr. Beddoes, who had previously witnessed it several times.

During the combination of lime with the mineral acids, a flash of white light is uniformly perceived; the same effect is not produced during the combination of strontian and barytes with these acids.

This phenomenon appears to be owing both to the attraction of the acids, and to the repulsive motion generated during the combination, a motion sufficient to give to the combined light, repulsive projection; for lime and magnesia become luminous when heated, which is not the case with strontian and barytes.

It is probable that some of the combustible bodies are phosphorescent. From an experiment of the Dutch chemists it appears that sulphur in its common state contains light. This experiment, which has been the subject of much speculation, has been several times repeated by Mr. Clayfield,* (and once

* It is to be hoped that this ingenious chemist will soon publish a particular account of it.

in my presence) with results different from those mentioned by the discoverers. When copper and sulphur cautiously freed, and included from phosfoxygen (oxygen gas) were made to combine by the heat of an argand lamp, a luminous appearance was perceived during the combination, and a considerable quantity of an inflammable gas, mingled with sulphureous acid, was liberated.

Phosphorus appears to contain light. Mr. Clayfield has often made me observe, during the combination of phosphorus and the earths, a fine vivid light, wherever the phosphoric vapor came in contact with the earths heated red.

The supposition that the inflammable bodies are phosphorescent, may in some measure reconcile the phlogistic and pneumatic theories with each other.

The phosphorescence of certain insects and putrifying animal substances will be treated of in the theory of respiration.

All the above mentioned phænomena equally take place in any gas, and appear to be perfectly independent of combustion.

Of PHOSOXYGEN, or OXYGEN GAS.

This gas (first discovered by the immortal Priestley) the great Lavoisier supposed to be oxygen combined with caloric, and on this supposition his theory of combustion is founded. The non-existence of caloric, or the fluid of heat, has been proved, and the materiality of light demonstrated.

Light is liberated during the oxygenation of certain bodies, as the following experiments will prove.

EXPERIMENT V.

The repulsive motion of carbon in contact with phosxygen (oxygen gas) was increased by a burning glass, till it became luminous; the carbon was rapidly diminished with the libera-

tion of a great quantity of light ; the temperature of the glass globe containing the phosphygen (oxygen gas) was very much increased, and a very small diminution of weight* was perceived. The phosphygen (oxygen gas) and carbon were almost entirely consumed, and a quantity of carbonic acid was formed, apparently nearly equal to the carbon and phosphygen.

EXPERIMENT VI.

The temperature of phosphorus in contact with phosphygen was raised by a burning glass, it immediately became luminous. An immense

* The following mode of ascertaining the diminution of weight was adopted in these experiments. The combustible body was suspended in a small earthen cup, in a glass globe filled with phosphygen. This globe was heated to dry it perfectly, attached to a balance, and accurately weighed before combustion. The combustible body was fired by a burning glass. During combustion the globe always rapidly ascended from the rarefaction of the surrounding atmosphere. After combustion, when the common temperature was restored, there appeared an evident small diminution of weight. This diminution was too small to be exactly ascertainable by the balance I made use of, and appeared to be different in different processes.

quantity of light was liberated, and the temperature of the surrounding bodies very much increased. A deficiency of weight was observed, a deficiency more considerable than I have found in any other combustible process; and phosphoric acid nearly equal to the phosphygen and phosphorus was formed.

EXPERIMENT VII.

Sulphur was heated in phosphygen. It rapidly diminished with the liberation of a great quantity of light, and great increase of temperature in the surrounding bodies. After combustion a small deficiency of weight was found; and sulphuric acid nearly equal to the sulphur and phosphygen employed, was formed.

EXPERIMENT VIII.

The temperature of hydrogen in contact with phosphygen was raised. The gases were diminished with great liberation of light, and great increase of temperature; and water nearly equal to them was formed.

EXPERIMENT IX.

Zinc was heated in contact with phosxygen ; it became luminous, and was consumed with a white brilliant flame. The substance remaining after combustion was white oxyd of Zinc.

EXPERIMENT X.

A small gun lock, armed with an excellent flint, was snapped in a vessel filled with phosxygen. The sparks of light arising from the particles of steel separated by collision, were the most brilliant that can be imagined ; and these particles examined by a magnifier, were found converted into black oxyd of iron.*

From these experiments, it appears that in the chemical process of the formation of many oxyds and acids, light is liberated, the phosxygen and combustible base consumed, and a new body formed, with properties essentially

* This experiment compared with the first, will afford a strong proof, not only of the composition of phosxygen, but likewise of the doctrine of repulsive motion.

different from the substances entering into the combination. Since light is liberated in these processes, it is evident, that it must be liberated either from the phos oxygen, or from the combustible body. It appears, as we have before said, that small portions of light are contained in some of the combustible bodies; but it appears to be accidental, and common to them with incombustible bodies; for their properties are not apparently altered when it is driven from them by increased repulsive motion; and we have no reasons for supposing that carbon, hydrogen, or any of the metals contain the smallest portions of light.

If the light liberated in combustion be supposed (according to Macquer's and Hutton's theories) to arise from the combustible body, then phos oxygen must be considered as a simple substance; and it follows on this supposition, that whenever phos oxygen combines with combustible bodies, either directly or by attraction from any of its combinations, light must be liberated, which is not the case, as carbon,

iron, and many other substances, may be oxyd-
ated by the decomposition of water, without
the liberation of light.

These experiments will appear more conclu-
sive when the synthetic experiments are con-
sidered, and the whole theory examined. It
appears that whenever bodies simply combine
with oxygen, light is liberated. There are
bodies that combine with oxygen and portions
of light, as will be hereafter proved, and others
that combine with phosphygen.

Combustion is a complex chemical process.
The decomposition of phosphygen by the at-
traction of a body for oxygen. The light is
generally liberated in the repulsive projectile
form, and the oxygen combines with the at-
tracting body to form an oxyd or acid. The
great increase of temperature arises from the
diminution of capacity in the combining bodies,
from the repulsive motion generated during the
combination, and from the concentrated liber-
ated light.

It is probable that there are some decompositions so slow, that the generated repulsive motion is not sufficient to give to light repulsive projection. A few metallic oxydations appear to be of this nature.

The light liberated in different combuſtive proceſſes, aſſumes very different appearances. During the combuſtion of bodies that remain ſolid or fluid at the temperature of combination, the ſenſation given by the liberated light is that which has been called a red or white heat. If the combuſtion proceeds but ſlowly, the red heat is produced, if more rapidly, the white. In the combuſtion of bodies that are gafeous, at the temperature of combination, the ſenſation known by the name of flame is produced by the liberated light. Theſe different ſenſations depend, moſt probably, on the rapidity of combuſtion. The light muſt be liberated ſlowly when phoſphorus is decompoſed by ſolids or fluids, and much more rapidly in the decompoſition of gaſes.

The differences in the colors of the liberated

light, must arise from different repulsive velocities given to the particles.

Since light and heat are totally distinct, it is evident that the names red and white heat, are improper, as applied to different modifications of light. Philosophy demands a more unequivocal nomenclature. The red and white heat might be called red and white slowly liberated light. As flame is a single word that can signify nothing but a modification of light, it may with propriety be retained in physical language.—To explain the phænomena of combustion, on their theory, the phlogistians were obliged to consider all combustible bodies as combinations of different unknown simple substances, with the undemonstrated phlogiston; and phosfoxygen, or vital air, a simple substance. This theory tended to confuse science, by referring to many unknown substances, to account for phænomena which evidently depend upon known ones. The phænomena of combustion, and the generated increase of temperature, are easily explained on the theory of

repulsive motion, and on that of the composition of phos oxygen, and only one unknown principle is admitted, namely oxygen, which we have never been able to obtain in its simple state, on account of its strong attraction for light and other substances ; but whose existence is perfectly demonstrable.

The theory of the immortal Lavoisier and the ingenious French nomenclators will ever be admired by chemical philosophers. It appears, indeed, to be possessed of these defects alone : the assumption of the imaginary fluid caloric, and the total neglect of light. According to the calorists, combustion ought always to take place when the gases are condensed ; but it is found that none of the gases, (except those of which phos oxygen is a component part) however rapid their combinations or decompositions, produce the smallest combustion, or liberation of light, when ammonia and carbonic acid (whose united capacity is much greater than that of phos oxygen and phosphorus) combine, no combustion is pro-

duced, and the increase of temperature is less than might be expected from so great a contraction of volume.

The proofs of the composition of phos oxygen founded on synthetical experiments, are no less conclusive than those deduced from the analytical ones. *It will appear that the presence of light is absolutely essential to the production of phos oxygen from pure oxyds and acids.*

Of those substances that have been heretofore distinguished by the common name of oxygen attractors, there are some, as has been before said, that wholly decompose phos oxygen by attracting the oxygen, with which they combine, whilst the light is liberated. These oxyds are difficult of decomposition by light alone, as is reasonable to suppose, and if at all, at a much higher temperature than of that of their combination with oxygen. The oxyd of lead is decomposable with less light, and with less increase of repulsive motion, than probably any of the other oxyds, as will appear from the following observation.

OBSERVATION *a.*

When pure oxyd of lead is heated as much as possible included from light, it remains unaltered ; but when exposed to the light of a burning glass, or even of a candle, phos oxygen is generated, and the metal revived.

In this process it is necessary that the temperature of deoxydation be greater than that of oxydation, as oxygen, at a certain temperature, has a stronger attraction for lead than for light ; but at a higher temperature, it attracts light stronger than lead.

But few experiments have heretofore been made on the revivification of metallic oxyds by the simple application of heat and light. In the common processes, they are restored by placing them in contact with bodies that have a stronger attraction for oxygen.

From the observation on the pure oxyd of lead, and those which have been made on the other oxyds, it appears that light is absolutely

essential to the generation of phosfoxigen from pure oxyds, and that phosfoxigen is never produced from them, but when light is present.

The substances that partially decompose phosfoxigen, that is, that combine with oxygen and portions of light, are more easily decomposable by light. The attraction between the base and oxygen is weakened by the attraction of light for oxygen ; and the addition of a small quantity of light effects the decomposition ; phosfoxigen is formed, and the phosfoxidable base remains pure, as the following observations will prove.

OBSERVATION *b.*

Oxygenated muriatic acid is a compound of muriatic acid, oxygen, and light, as will be hereafter proved. The combined light is not sufficient to attract the oxygen from the base to form phosfoxigen ; but its attraction for oxygen renders the acid easily decomposable. If this acid be heated in a close vessel, and light excluded, no phosfoxigen is formed ; but if it

be exposed to the solar light, phosfoxygen is formed, the acid loses its oxygen and light, and becomes muriatic acid.

Now since light, by producing repulsive motion cannot, as is evident from the first part of the last experiment, decompose oxygenated muriatic acid, it is evident that it must act chemically, that is, by combination; it must attract oxygen and light from the acid, and this combination is phosfoxygen.

OBSERVATION *c.*

If nitric acid, which is compounded of oxygen, light, and nitrogen, (as will be hereafter proved) is exposed to the solar light, phosfoxygen is formed, and the acid reduced to the state of nitrous acid, that is, is deprived of a portion of its oxygen and light.

OBSERVATION *d.*

The yellow oxyd of tungsten consists of a peculiar metallic base, oxygen, and probably a small portion of light. If this oxyd be exposed

to the solar light, phosfoxygen is produced, the oxyd loses weight, and becomes blue.

OBSERVATION *e.*

The green prussiate of iron, exposed to the solar light, gives out phosfoxygen, and becomes blue.

OBSERVATION *f.*

If the oxyds of gold or silver be exposed to the solar light, phosfoxygen is produced, and the metals deoxydated.

From these observations it appears that light is essential to the production of phosfoxygen from oxyds and acids; and the quantity of light essential, appears to be inversely proportional to the quantity contained in the combination. The substances that contain portions of light combined with their oxygen, are easily revived by a small quantity of light, and a small increase of repulsive motion. The compounds, on the contrary, that are pure oxyds, that is, which contain no light, as the oxyds of

iron, nickel, &c. require large portions of concentrated light, and a great increase of temperature, to produce from them phosfoxigen.

Certain combinations of oxygen cannot be decomposed by the simple elective attraction of light for oxygen. These require for their decomposition the united force of two attractions: that of light for oxygen, and of some substance for the oxydable base.

Among these substances are water and carbonic acid.

Water, as is proved by the tenth experiment, is composed of oxygen and hydrogen. When the oxygen of phosfoxigen combines with hydrogen to form water, light is liberated. Water is decomposed by two attractions; that of light for oxygen, and of a certain hydrogen attractor for hydrogen.

The marine cryptogamiæ, vegetables, and a number of other substances, attract hydrogen.

That the marine cryptogamiæ* attract hydrogen, is evident from their analysis. I have found that they afford, when decomposed by repulsive motion, amongst other products, a considerable quantity of hydrogen. This hydrogen, as they are nourished entirely by water, or by substances held in solution by water, it is reasonable to suppose they somehow gain from the decomposition of water. To remove all doubt, however, concerning the attraction of the marine cryptogamiæ for hydrogen, I made the following experiment.

EXPERIMENT XI.

One cubic inch of *conserva fæniculacea* was put into a vessel containing thirteen cubic inches of hydrogen. It remained in a temperature of 58° for six hours, and at the end of that time was examined. The hydrogen was diminished

* I have preferred giving an account of the decomposition of water by the marine cryptogamiæ, to that effected by the vegetation of land plants, both as it is a fact heretofore unknown, and as, from the inferiority of their organic powers, their chemical attraction may be more readily admitted.

eight tenths of a cubic inch. I could get no ballance sufficiently accurate to determine the weight gained by the plant.

Asthemarine cryptogamiæ cannot be obtained perfectly dry in their vegetative state, it was necessary to prove that the hydrogen gas diminished was not absorbed by the water in contact with the plant. For this purpose two phials, containing each $13\frac{1}{2}$ cubic inches, were filled with hydrogen. One cubic inch of *conferva foeniculacea* was inserted into the one, and two cubic inches of wool, previously wetted, into the other. The quantity of gas in each of them was then accurately determined. The phials were inverted in the same vessel of water, and at the end of twelve hours examined. The diminution of the *conferva* was near a cubic inch and quarter; by the wool and water, not more than three-tenths of an inch.

This experiment proves that the marine cryptogamiæ attract hydrogen; but their attraction is weaker than the attraction of oxygen

for hydrogen, as it is found that seaweeds, or vegetables of any kind, placed in contact with water, and deprived of light, effect no alteration in it as long as they retain life. Water, consequently, is not decomposed by the simple elective attraction of bodies for its hydrogen.

The attraction of a body for hydrogen does not effect the decomposition of water, when assisted by an increase of repulsive motion, as the following experiment will prove.

EXPERIMENT XII.

One hundred and four cubic inches of water, previously boiled to expel the atmospheric air, were heated with three cubic inches of *conferva fusca*, in a vessel from which light was excluded. The temperature was gradually raised to 200° ; but not more than a few globules of gas were formed, which by trial with nitrous gas proved to be of the same quality as atmospheric air.

Water exposed to the solar light in contact

with the marine cryptogamiæ, or any organized hydrogen attractor, is decomposed by the force of two attractions; that of the hydrogen attractor for hydrogen, and of light for oxygen, as the following experiment will prove.

EXPERIMENT XIII.

Into a green glass globe containing 214 cubic inches of sea water, previously boiled to expel the atmospheric air, one cubic inch of *conferva littoralis* was inserted. The globe was inserted in a jar of water of a similar kind, and exposed in a bright sunshine for four hours. In this time five cubic inches of gas were formed; which by trial with nitrous gas, proved to be $\frac{71}{100}$ parts phos oxygen, and $\frac{29}{100}$ azote.* The next day the globe was exposed to a bright sunshine for three hours. Two cubic inches, and five tenths were pro-

* By azote, I mean a gas incapable of diminution with nitrous oxyd; I have always called the gas to which the French nomenclators give the name of azote, nitrogen, after Chaptal, and some English chemists.

duced, which by trial with nitrous gas, proved to be $\frac{76}{100}$ phosfoxigen, and $\frac{24}{100}$ azote. The day after, the globe was exposed for five hours; but the sky was often clouded, and there fell some showers. Three cubic inches were formed, of the quality of $\frac{86}{100}$ phosfoxigen and $\frac{14}{100}$ azote. After this, all the gas formed, contained from $\frac{76}{100}$ to $\frac{86}{100}$ of phosfoxigen. The other gases liberated with the phosfoxigen, appeared to be nitrogen and carbonic acid: at the latter part of the experiment, there was some indication of the presence of hydrogen. The whole quantity of gas produced from 214 cubic inches of water, in thirty-six hours of sunshine, was 46 cubic inches of the mean quality of $\frac{80}{100}$ phosfoxigen, and $\frac{20}{100}$ of a gas or gases indiminifhable by nitrous oxyd.

I have made a number of experiments on the decomposition of water and carbonic acid by the marine cryptogamiæ, &c. the particulars of which it is unnecessary to mention here. I shall give an account of them in an essay on the generation of phosfoxigen. A few observations

made in the course of these experiments, will I think, afford additional proofs of the theory delivered in this essay; and therefore I shall relate them.

First, An increased or diminished temperature produced no sensible difference in the production of gas.

Secondly, A very strong artificial light acted similarly in the production of gas, to the solar light.

Thirdly, The capillary, dark, coloured, and opaque conservæ, generated more and better gas, than the white or pellucid. The conservæ generated more and better gas than the ulvæ, and the ulvæ more than the fuci.

Carbonic acid is not decomposable by light alone. No alteration is effected in it, though exposed for any length of time to the solar rays; nor is it decomposable by vegetables; which, as is evident from their analysis, attract carbon

in large quantities. These facts I have proved by experiments of which an account will be given in an essay on the generation of phos-oxygen.

Carbonic acid is decomposable by the force of two attractions ; that of any vegetable base, or carbon attractor for its carbon ; and that of light, for its oxygen, as the following experiment will prove.

EXPERIMENT XIV.

A plant of *Arenaria Tenuifolia* planted in a pot filled with very dry earth, was inserted in carbonic acid, under mercury. The apparatus was exposed to the solar light, for four days successively, in the month of July. By this time the mercury had ascended considerably. The gas in the vessel was now measured. There was a deficiency of one sixth of the whole quantity. After the carbonic acid was taken up by potash, the remaining quantity equal to one seventh of the whole, was phos-oxygen almost pure. From this experiment, of which a further detail

will be given in an essay on the generation of phosfoxygen, it is evident that carbonic acid is decomposed by two attractions; that of the vegetable for carbon, and of light for oxygen: the carbon combines with the plant, and the light and oxygen combined are liberated in the form of phosfoxygen.

Thus we have direct synthetical as well as analytical proofs of the composition of phosfoxygen. It has been demonstrated then, *that phosfoxygen is light combined with oxygen.*

I have heretofore possessed no balance sufficiently accurate to determine exactly the deficiency of weight from the light liberated in different combustive processes. It is probable that light is liberated in an imperceptible form in some combustions unaccompanied with great generation of repulsive motion. In these processes we can determine its liberation only from the deficiency of weight perceived.

It would probably throw much light upon

the properties of the different oxyds and acids to determine the quantity of light entering into their composition. The active properties of this substance may in a great measure influence the effects produced by the bodies into whose composition it enters on the organs of sense.

If it were possible to determine with accuracy the deficiency of weight in combustion arising from the liberated light we might be able to discover the quantities entering into the composition of the acids.

Independant of the great use of phosoxygen, as the pabulum vitæ of organic beings, it is of the greatest importance to man simply considered as the supplier of light and heat by combustion. On the decomposition of phosoxygen by combustible bodies the greater portion of the comforts and luxuries of life depends. Without combustion man might have wandered for ever barbarous and uncivilized in his native desarts. By the help of combustion the artist and manufacturer fabricate the tools, by which we erect

cities, subdue and cultivate the earth, and directly derive our support. Assisted by combustion Commerce erects the stately vessel, subjugates the ocean, showers plenty over every nation, and connects mankind together. By the arts, dependant on combustion, science and philosophy no longer confined to thinking individuals, exist in characters. The press has made them immortal, and will ever continue to extend their beneficial influence. And lastly, aided by combustion, the sage devotes to philosophy the solitary hours of midnight, pursuing those combinations of ideas, which producing inventions improve and ameliorate the condition of man.

I am inclined to believe from some circumstances, observed in the course of these experiments ; that light and oxygen combine in different proportions. This opinion at present I am not able to demonstrate experimentally ; but I think it is countenanced by a number of facts. The phænomena observed in the higher regions of the atmosphere render this supposi-

tion extremely probable. Respiration is painful on the tops of high mountains, and an inflammatory state of the system is induced; combustion is carried on with greater facility and at a lower temperature than on the plain, as M. Saffure found that carbon caught fire sooner and burnt quicker on the top of the Alps than on the plain beneath. Fiery meteors appear at an amazing height in the atmosphere, much beyond that at which the solar rays are refracted;* and these meteors most probably arise from the formation of water.—We cannot account for these phænomena on any common principles. Phosfoxygen and Nitrogen are intimately combined here below; and they expand, when acted on by an increased repulsive motion in the same ratio. Now as the volumes of elastic fluids are in the inverse ratio of their compressing weight; if phosfoxygen and nitrogen be supposed to compose the whole of our

* One of these was computed by Dr. Halley to be above ninety miles high.

atmosphere, then must the quantity of phosfoxygen in the atmosphere decrease in proportion as the height increases.

On this supposition the phænomena are totally inexplicable. For a deoxygenated*atmosphere, instead of generating an inflammatory state of the system, tends to diminish it ; combustion is carried on with ease and rapidity in proportion as the quantity of phosfoxygen is greater ; and if the atmosphere at 90 miles high be supposed to be composed of eminently rarified air, it is almost impossible that combustion could be carried on there. These difficulties can be got over with ease on the supposition, that light and oxygen combine in different proportions. Light, continually acting upon the phosfoxygen of the atmosphere, may combine with portions of it, and form a luminated phosfoxygen ; which must necessarily be

* Nor will the subtraction of pressure from the vessels account for this inflammatory state of the system, as action and reaction are equal.

of less specific gravity, and easier of decomposition than phosxygen ; and this gas, from its small specific gravity, and probably still farther combinations with light, may extend to an amazing distance from our planet.

The higher regions of the atmosphere being supposed to be filled with this gas, combustion must take place on the tops of mountains at a lower temperature than on the plain, and with a greater liberation of light ; because the phosxygen there is combined with a greater proportion of light. Respiration must become painful, and an inflammatory state of the system be induced ; because the blood becomes supersaturated with light ; which as will be proved in the theory of respiration, is probably in a peculiar manner one cause of inflammation. The rays of light are not refracted in the atmosphere above 45 miles high, because beyond that the atmosphere is amazingly rare, being composed of phosxygen highly luminated. Hydrogen ascends in the atmosphere, till it comes in contact with highly luminated phosxygen pro-

bably of the same specific gravity : the oxygen loosely adhering to the light is attracted from it by the hydrogen at the common temperature of the atmosphere to form water, whilst the light is liberated, and hence the phenomena of fiery meteors at a very great height.—I have invented experiments for the investigation of this opinion ; and I hope to be soon possessed of the means for their execution.

From the great quantity of light liberated in many combustive processes, particularly those in which the phosphoric, sulphuric and carbonic acids and water are formed; we may fairly conclude that phosxygen is wholly decomposed in these processes. The light liberated and the oxygen attracted by the base ; yet there are others, as will be by and by proved, in which there is only a partial decomposition of phosxygen. In these only a portion of light is liberated, whilst the other part united to the oxygen combines with the attracting base. And phosxygen (oxygen and light) often combines with bodies, without decomposition.

These substances, which are now to be treated of, are combinations of light, oxygen and bases.*

We have been obliged to form a new nomenclature for the combinations of phosfoxigen; neither that of the phlogistians, or of the calorists, would express their composition with accuracy. On the modern principles of chemical nomenclature all compound substances should be distinguished by names characteristic of the substances forming the compound. We have endeavoured to adhere to this plan. All the combinations of phosfoxigen that have acid properties, are denoted by the names of phos-acids; those which have no acid properties, are called phosoxyds. By these terms the com-

* From the experiments related by chemical writers on the metallic oxyds, one can draw no certain conclusions concerning the light liberated in oxydation. The peculiar properties of these bodies and their uses, have been more attended to than the process of oxydation.—I intend as soon as an opportunity offers to engage in a set of experiments on oxydation.

pounds of light, oxygen, and bases, will be sufficiently distinguished from the combinations of oxygen and bases which are simply called oxyds and acids.

The terminations *ous* and *ic*, after the principles of the French nomenclators, will signify the different quantities of phosfoxigen entering into the composition of the phosoxyds and phosacids. The names of the acidifiable and oxydable bases are nearly the same as in the French nomenclature. We have substituted nitrogen for azote, after Chaptal, Pearson, and some other chemists.

COMBINATIONS of PHOSOXYGEN.

<i>Substances that combine with Phosxygen.</i> <i>Combinations of PHOSOXYGEN and Substances.</i>		
<i>New and Old Names.</i>	<i>New Names.</i>	<i>Old Names.</i>
Nitrogen	{ 1 Nitrous Phosoxyd 2 Nitric Phosoxyd 3 Nitrous Phosacid 4 Nitric Phosacid	{ 1 Gaseous Oxyd of Azote 2 Nitrous Gaz 3 Nitrous Acid 4 Nitric Acid
Muriatic Acid	Muriatic Phosacid	Oxygenated Muriatic Acid
Platina -	Platinic Phosoxyd	Oxyd of Platina
Gold -	Auric Phosoxyd	Oxyd of Gold
Silver -	Argentinc Phosoxyd	Oxyd of Silver
Mercury -	Mercuric Phosoxyd	Red Oxyd of Mercury
Lead -	Plumbic Phosoxyd	Red Oxyd of Lead
Tungsten -	Tungstic Phosoxyd	Yellow Oxyd of Tungsten
Manganese	Manganefic Phosoxyd	Oxyd of Manganese
Chrome -	Chromic Phosacid	Acid of Chromic*
Cobalt -	Cobaltic Phosoxyd	Rose colored Oxyd of Cobalt
Other Metals	Quere	Quere

* This Acid has been lately discovered by Vauquelin in the red lead of Siberia.

**COMBINATIONS of the NITROUS and NITRIC
PHOSACIDS with SUBSTANCES.**

<i>Substances that combine with the Nitrous and Nitric Phosacids.</i>		<i>Combinations of the NITROUS and NITRIC PHOSACIDS with Substances.</i>	
<i>New and Old Names.</i>		<i>New Names.</i>	<i>Old Names.</i>
Barytes -	{	Phosnitrite } Phosnitrate } of Barytes	Nitrite } Nitrate } of Barytes
Strontian -	{	Phosnitrite } Phosnitrate } of Strontian	Nitrite } Nitrate } of Strontian
Potash -	{	Phosnitrite } Phosnitrate } of Potash	Nitrite } Nitrate } of Potash
Soda -	{	Phosnitrite } Phosnitrate } of Soda	Nitrite } Nitrate } of Soda
Lime -	{	Phosnitrite } Phosnitrate } of Lime	Nitrite } Nitrate } of Lime
Magnesia -	{	Phosnitrite } Phosnitrate } of Magnesia	Nitrite } Nitrate } of Magnesia
Ammonia -	{	Phosnitrite } Phosnitrate } of Ammonia	Nitrite } Nitrate } of Ammonia
Argilla -	{	Phosnitrite } Phosnitrate } of Argilla	Nitrite } Nitrate } of Argilla
Metallic Oxyds and Phosoxyds	{	Phosnitrites } Phosnitrates } of Metals	Nitrite } Nitrate } of Metals

COMBINATIONS *of* PHOSOXYGEN.COMBINATIONS *of* NITROGEN
with PHOSOXYGEN.

Nitrogen forms seventy-two hundred parts of the air of our atmosphere. With regard to the present state of our knowledge it must be considered as an undecomposed substance.

It enters into combination with a number of bodies. In organic compounds it is found in considerable quantities; and appears to act an important part in the phenomena of life. Phos-oxygen and nitrogen combine in different proportions, and forms substances possessing specifically different properties.

When phosoxxygen and nitrogen are made to combine by the action of the electric spark, it appears that no light is liberated in the pro-

cess. In this experiment phosnitric acid is formed. And as phosxygen is compounded of light and oxygen, and nitrogen is a simple substance, it is evident from this experiment that phosnitric acid is a compound of light, oxygen and nitrogen. The proof from analytical experiment is even more conclusive, and will account for a phenomenon which the other theories were inadequate to explain.

EXPERIMENT XV.

Phosnitrate of potash mingled with half its weight of carbon was fired by a burning glass in the exhausted receiver. The conflagration took place and a considerable quantity of light was liberated in the repulsive projectile state. The gaseous products were examined and proved to be nitrogen and carbonic acid. The fixed substance remaining after combustion was potash mingled with carbon. The quantities of the products were not accurately ascertained, as the end of this experiment was simply to determine their nature.

Nitric phosacid then is compounded of light, oxygen and nitrogen. From the experiments of Lavoisier we may conclude, that one hundred parts of it contain seventy-nine and a half of phos oxygen, and twenty and a half of nitrogen. When carbon heated to a certain degree is placed in contact with phosnitrate of potash it attracts the oxygen of the nitric phosacid, and combines with it to form carbonic acid : the light and nitrogen having no combining attraction for each other, or for potash, are liberated, one in the repulsive projectile and the other in the gaseous form. The great increase of repulsive motion is produced from the rapid divellent and combining chemical motions generated in the process. The detonation is occasioned by the undulatory motion generated in the circumambient atmosphere by the rapid dislodgment of a body of air equal in bulk to the elastic fluids generated in the process.*

* Lavoisier, and the Calorists, suppose this detonation to be occasioned in a great measure by the liberation of Caloric. They assert that when oxygen gas and azotic gas combine to form nitric acid, they retain in their composition a

The nitric phosacid is decomposable by increased repulsive motion alone, into nitrogen and phosoxygen.

It combines with water, with the alkalies, the alkaline earths and metallic oxyds, forming compounds formerly called nitrates ; but which to express their composition more accurately, we have called phosnitrates.

When nitric phosacid is exposed to light, it loses a portion of its oxygen and light, and becomes nitrous phosacid, as was before ob-

great quantity of the caloric which rendered them gaseous. This caloric they say is liberated in the decomposition of Nitrate of Potash, and hence, the increase of temperature, detonation, &c. This hypothesis is one of the most absurd advanced by the Calorists.—On their theory of caloric, it is evident, that when the temperatures of bodies are increased in chemical processes, their capacities must be diminished ; and therefore, the capacities of carbonic acid, azote and potash, must be much less than those of carbon and nitrate of potash, than which nothing is more false : for I have found by experiment, that the united capacity of nitrate of potash and carbon, is much less than that of carbonic acid or azote, and independent of this, they have totally neglected the liberated light.

served. One hundred parts of this acid appears to contain about seventy-four parts phosfoxigen, and twenty-six nitrogen. Like the nitric phosacid it is decomposable by increased repulsive motion into phosfoxigen and nitrogen; and when combined with bases, by certain heated oxygen attractors, into light, oxygen and nitrogen. The nitrous phosacid combines with water, with the alkalies, alkaline earths, metallic oxyds, &c. With the alkalies, alkaline earths, and metallic oxyds, it forms compounds which possess like the phosnitrates, the property of detonating, i. e. of being rapidly decomposed by heated oxygen attractors; to these substances we have given the names of phosnitrates.

When a considerable portion of oxygen and light is subtracted from the nitric phosacid by metallic substances, the gas liberated during the process is nitric phosoxyd.

This substance combines in small proportions with water, is a permanent gas at the common temperature of the atmosphere, and appears to

contain about sixty-eight per cent phosfoxygen, and thirty-two nitrogen. Phosphorus decomposes it by attracting the oxygen to form phosphoric acid, whilst the light and nitrogen are liberated.

Dr. Priestley discovered, that when nitric phosfoxyd (nitrous air) is exposed to the action of moistened iron filings for a certain time, a diminution of its volume takes place, and a gas is formed, possessing peculiar properties, capable of supporting the flame of a candle better than atmospheric air; but at the same time totally unfit for the respiration of animals. To this gas he gave the name of dephlogisticated nitrous air; but we have called it from its composition, nitrous phosfoxyd. It appears to contain less oxygen, and a larger proportional quantity of light than nitric phosfoxyd, as will appear from the following observations :

OBSERVATION *g*.

When nitric phosfoxyd is exposed to the action of heated iron for a certain time, the

iron becomes oxydated, and nitrous phosfoxyd is formed. No light is liberated during the process. The oxyd of iron formed in this manner, is in every respect similar to that formed by direct decomposition of phosfoxygen.

OBSERVATION *b*.

When the repulsive motion of phosfnitrate of ammonia is increased to a certain degree, a new arrangement of its principles take place. Water and nitrous phosfoxyd are formed, and a portion of azote is liberated. No luminous appearance is perceived during this process. Now as nitric phosfoxyd and nitric phosfacid are compounded of light, oxygen, and nitrogen, and according to the foregoing observations, no light is liberated during the formation of nitrous phosfoxyd, it is evident, that it must be composed of nitrogen, light, and a smaller portion of oxygen. The Dutch chemists have concluded from experiments on its decomposition, that one hundred parts of it contain thirty-seven and half oxygen, and sixty-two

and half nitrogen. The light entering into its composition, has never been attended to by any chemist. This gas combines in very small proportions with water, and appears to possess no acid properties. It is decomposable by hydrogen and by certain combinations of hydrogen and carbon. Sulphur, phosphorus and carbon, appear incapable of attracting oxygen from it at any common temperature. I have found by experiment, that a candle burns in this gas with a flame larger and more brilliant than in a gas composed of a mixture of thirty-eight parts phosphygen, and sixty-two nitrogen ; which alone would prove that it contained a larger proportional quantity of light, than any of the other combinations of nitrogen and phosphygen. I have made some other experiments on the composition of this gas, and some on the effects produced by it on animals, which will be detailed in a distinct essay.

It is extremely probable that the air of our atmosphere is a chemical combination of phosphygen and nitrogen. A mixture of twenty-eight

parts of photoxygen, and seventy-two of nitrogen, is not exactly similar to the air of our atmosphere. We want however experiments to determine the truth or falsehood of this supposition. If the fact was proved, this gas might be called photoxyd of nitrogen.

**COMBINATIONS of the MURIATIC PHOSACID
(OXYGENATED MURIATIC ACID.)**

<i>Substances that combine with Muriatic Phos acid.</i>	<i>Combinations of the MURIATIC PHOSACID with Substances.</i>	
<i>New and Old Names.</i>	<i>New Names.</i>	<i>Old Names.</i>
Barytes -	Phosmuriate of Barytes	Oxygenated Muriate of Barytes
Strontian -	Phosmuriate of Strontian	Oxygenated Muriate of Strontian
Potash -	Phosmuriate of Potash	Oxygenated Muriate of Potash.
Soda -	Phosmuriate of Soda	Oxygenated Muriate of Soda.
Ammonia -	Phosmuriate of Ammonia	Oxygenated Muriate of Ammonia. *
Lime -	Phosmuriate of Lime	Oxygenated Muriate of Lime.
Magnesia -	Phosmuriate of Magnesia	Oxygenated Muriate of Magnesia.
Argilla -	Phosmuriate of Argilla	Oxygenated Muriate of Argilla
Metallic Oxyds and Phoxyds. }	Phosmuriates of Metals.	Oxygenated Muriates of Metals.

* Quere?

Of the MURIATIC PHOSACID.

The muriatic acid has been long discovered. Analogy would induce us to suppose that it is a compound of oxygen with some acidifiable base ; but we are at present possessed of no facts sufficient to prove its composition. We have attempted to decompose this acid by passing phosphoric vapor through muriate of lime strongly heated ; but no phosphoric acid was formed, and the muriate of lime remained unaltered. The muriatic acid combines with phosfoxygen, and forms an acid possessing peculiar properties. To this acid discovered by Scheele, the French nomenclators have given the name of oxygenated muriatic acid, on the supposition that it was muriatic acid combined with oxygen. We have called it muriatic phosacid, to express the combination of Light, Oxygen, and Muriatic Acid. The following experiment will prove analytically that the mu-

riatic phosacid is a compound of light, oxygen, and muriatic acid.

EXPERIMENT XVI.

Phosmuriate of potash was mingled with twice its weight of carbon, and fired by a burning glass in the exhausted receiver. The detonation took place with great increase of temperature in the surrounding bodies. A great quantity of brilliant repulsive projectile light was liberated. The gaseous products, on examination, proved to be carbonic acid and muriatic acid, the fixed substances remaining after combustion, were carbon, potash, and a small quantity of muriate of potash.

From this experiment it is evident that the muriatic phosacid is compounded of light, oxygen, and muriatic acid. Phosmuriate of potash is compounded of phosmuriatic acid and potash. When carbon is placed in contact with this substance, and heated, it attracts the oxygen of the muriatic phosacid stronger than it is, attracted by the light and muriatic acid, and

combines with it to form carbonic acid. The light and muriatic acid having no affinity for each other, are liberated. The great increase of repulsive motion generated in this process arises from the rapid motions of the combining and liberated bodies.

The composition of the muriatic phosacid may be proved by synthesis, as well as analysis; for muriatic acid is never phosoxygenated; but by combining with phosoxygen, or by attracting it from some of its combinations, as will be better understood hereafter.

The muriatic phosacid is decomposable by light, as was proved by observation *b*. It is likewise decomposable by phosphorus, sulphur, carbon, and metallic substances, when their temperatures are slightly increased by friction or percussion.*

* To account for these detonations, the French chemists were obliged (as in the case of the nitric phosacid) to suppose an immense quantity of caloric in the composition of this acid, which is directly contradictory to Black's doctrine of capacity.

The muriatic phosacid combines with potash, soda, the alkaline earths, metallic oxyds, phosoxyds, &c. The phosmuriate of potash appears to contain in its composition a still larger quantity of oxygen and light than the muriatic phosacid; for during the combination of the muriatic phosacid with potash, a certain quantity of muriatic acid is found in combination with a portion of the potash. And when sulphuric acid is poured on phosmuriate of potash, light, phosoxygen, and phosmuriatic acid gas are liberated.* The phosmuriate of soda possesses similar properties to the phosmuriate of potash, with greater solubility in water. The other combinations of the phosmuriatic acid have not heretofore been much attended to. Berthollet

* We thought it probable, from the phenomenon accompanying the decomposition of the phosmuriate of strontian, about to be described, that the same effects would be produced when the phosmuriate of potash was used. Sulphuric acid, of the specific gravity of 2,25, was poured on a few grains of phosmuriate of potash: vivid white light was instantly liberated, and phosoxygen and phosmuriatic acid gas given out with great rapidity.

found that it was incapable of combination with ammonia at the common temperature; when mingled with this substance, a double decomposition takes place, and water, muriatic acid, and nitrogen, are the products. We have succeeded in combining this acid with strontian.*

* I effected this combination by passing muriatic phosphoric acid gas through a saturated solution of strontian lime heated above 200°. The strontian lime was obtained by my friend Mr. Clayfield, from the sulphate of strontian discovered by him near Bristol. We first attempted the combination by passing muriatic phosphoric acid gas through strontian lime water at the temperature of 30°,—40°; but without success. Mr. Clayfield proposed to try the effect of cold. The temperature of the solution was lowered by snow and salt to 10°, and the gas passed through; but no considerable combination was effected. We then dissolved as much earth as possible in boiling water, and passed the gas through the saturated solution. The combination immediately took place. The solution of phosphomuriate of strontian was of a dusky orange color. We had some difficulty in obtaining the crystals of this salt, from its extreme solubility. By great evaporation and cooling, it gave fine needle formed crystals. These crystals slightly detonated with phosphorus and charcoal. Alcohol holding them in solution, burnt with a rose colored flame. When sulphuric acid was poured into a solution of this salt in water, with a design to prove its composition by synthesis, a beautiful and unexpected phenomenon took place. The room was accidentally darkened at the moment that this experiment

The phosmuriate of strontian possesses extreme solubility; its chrySTALLIZATION is similar to that of the muriate of strontian, and like that salt, when dissolved in alcohol, it communicates to it the property of burning with a rose colored flame. It detonates slightly with carbon and phosphorus. During the decomposition of this salt by the sulphuric acid, a beautiful phenomenon takes place, the liberation of variously colored light.

It is not improbable that by attending to the combinations of the muriatic phosacid, we may

was made, so that we were enabled to perceive a vivid luminous appearance whilst muriatic phosacid gas was liberated with great increase of temperature. We repeated the experiment two or three times with the same result, except that the light was differently colored. When sulphuric acid of the specific gravity of 2,25 was poured on the dry salt, no light was liberated, and the decomposition went on very slowly; on the addition of water, the effects before described again took place. This experiment, independent of its beauty, is extremely pleasing, as affording an instance of a true combustion, that is, the production of light and heat, by the mixture of two incombustible bodies.

discover some cheap substitute for the phosphinate of potash. From the present consumption of this substance in the murder of mankind, the nitric phosphoric acid is an extremely expensive article. The great importance of this acid in chemistry and the arts, renders the cheaper acquisition of it a great desideratum.

COMBINATIONS of LIGHT, OXYGEN, and
METALLIC SUBSTANCES.

Since the discovery of oxydation by the great Lavoisier, chemical philosophers in general have considered the process as the simple decomposition of oxygen gas (oxygen and caloric); the combination of oxygen with the oxydable base, and the liberation of caloric. All metallic oxydations have been conceived to be similar, and differing only in the rapidity of the decomposition of oxygen gas. As light has been little attended to, even among the chemical principles of the most celebrated pneumatists, we are not surprised to find that it has been neglected in the process of oxydation. The combinations of light, oxygen, and metallic bodies, have not been heretofore distinguished from the simple combinations of oxygen with these substances, though from the differences of their properties alone they might well be considered

as a distinct class of substances. To these bodies we have given the name of phosoxyds, to distinguish them from the simple combinations of oxygen, and to express their composition with accuracy.

PLATINIC PHOSOXYD.

Platina appears incapable of directly combining with light and oxygen at any known temperature. This combination is effected by the decomposition of the muriatic phosacid, or the murionitric phosacid. In the decomposition of the muriatic phosacid by platina, the platina combines with the light and oxygen, so as to convert the acid into muriatic acid. The properties of this phosoxyd are but little known, and I have never had an opportunity of examining them.

AURIC PHOSOXYD.

Gold, which is incapable of decomposing, or of directly combining with, phosxygen at any known temperature, possesses the power of at-

tracting it from some of its combinations. Gold becomes phosxydated by attracting light and oxygen from the muriatic phosacid or the murionitric phosacid; it is extremely probable, from the properties of this phosxyd, that it contains a larger proportion of oxygen, and a smaller proportion of light, than the other phosoxyds. Exposed to light, it gives out phosoxygen and the metal is revived. It combines with the acids, and with ammonia; with ammonia it forms the compound called fulminating gold, but which might be called with greater propriety, auric phosoxyd of ammonia. From the decomposition of this substance alone, we might prove that light was one of the constituent parts of auric phosoxyd. When the temperature of auric phosoxyd of ammonia is increased a little, it is decomposed with a great explosion, and the products are light, nitrogen, and water. In this process the hydrogen of the ammonia combines with the oxygen of the auric phosoxyd to form water, and the nitrogen of the ammonia, and the light of the phosoxyd, having no attraction for each other or for water, are liberated.

ARGENTIC PHOSOXYD.

Silver, like gold and platina, is incapable of combining with phosoxygen directly, at any known temperature. Silver appears capable of combining with oxygen, as well as phosoxygen, and consequently it decomposes both the acids and phosacids. It attracts oxygen and light from the nitric phosacid with great rapidity, and becomes phosoxydated, whilst the remaining light and oxygen of the acid fly off with the nitrogen in the form of nitric phosoxyd. The argentic phosoxyd combines with the acids, and with ammonia. Its combination with ammonia affords one of the most astonishing phænomena in chemistry. To form this substance, the phosoxyd of silver must be precipitated from its solution in nitric phosacid by lime-water. This precipitate after exposure to light for some hours, must be stirred in a solution of ammonia. When this solution is evaporated, the crystallised substance remaining is the fulminating silver, or argentic phosoxyd of ammonia. The

lightest possible change of temperature by friction, percussion, or any other means, causes this substance to explode with an astonishing detonation, and the liberation of light. The products of this detonation have never been examined. Light we know is one of them, the others are probably water and nitrogen: for the substance is a compound of light, oxygen, silver, nitrogen, and hydrogen. The lightest change of temperature disposes the light to fly off, and the hydrogen and oxygen to combine to form water. The great increase of temperature, and the detonation inexplicable on the former theories, are accounted for with the greatest ease on the theory of repulsive motion, without any absurd or unnatural suppositions.

MERCURIC PHOSOXYD.

Mercury combines directly with phosxygen, as appears from the following observations.

OBSERVATION *i.*

When mercury is placed in contact with phosxygen, and its temperature raised nearly

to its boiling point, the phosxygen and mercury are gradually converted into a red substance possessing properties essentially different from mercury or phosxygen. No light is liberated in this process. The phosoxyd of mercury is consequently a combination of light, oxygen, and mercury. This substance is likewise formed by the decomposition of the nitric phosacid. Its fine red color, like that of most other substances, appears to depend on the light entering into its composition. It combines with the acids, and with ammonia. With ammonia it forms a fulminating compound, the mercuric phosoxyd of ammonia.

The mercuric phosoxyd is decomposable by light and increased repulsive motion, into phosxygen and mercury.

PLUMBIC PHOSOXYD.

The plumbic phosoxyd (red oxyd of lead) evidently contains oxygen and light; for when the white oxyd of lead is heated in contact with

phosoxygen, it becomes red and more ponderous, and the phosoxygen is absorbed without the liberation of light. When muriatic acid is distilled from the plumbic phosoxyd, it becomes muriatic phosacid. From whence we may conclude that the plumbic phosoxyd is compounded of oxygen, light, and lead.

TUNGSTIC PHOSOXYD.

The tungstic phosoxyd (yellow oxyd of tungsten) appears to contain light in its composition. It gives out phosoxygen, and becomes blue on exposure to the solar light. It appears capable of combining with the alkalies, alkaline earths, &c.

MANGANESIC PHOSOXYD.

The manganefic phosoxyd (black oxyd of manganefe) evidently contains phosoxygen; it affords it on the application of heat. When muriatic acid is distilled from it (the manganefic phosoxyd,) it becomes muriatic phosacid, that

is, it subtracts a portion of light and oxygen from the manganefic phosoxyd.

CHROMIC PHOSACID.

This acid, just discovered by the ingenious Vauquelin, appears to contain light; its red color alone would induce us to believe this, and from the experiments of Vauquelin, it appears that it loses its red color when exposed to light, and most probably gives out a portion of light and oxygen.

COBALTIC PHOSOXYD.

The cobaltic phosoxyd is formed by the decomposition of the nitric phosacid.

We have found that it gives out phosoxygen when its repulsive motion is increased, from whence we conclude that it is a combination of light, oxygen, and cobalt.

The phosoxyds possessed of the most striking

properties are, the auric, argentic, mercuric, and plumbic. The facility with which these bodies are decomposed, is evidently dependant on the light entering into their composition. Indeed we have no single instance of a chemical detonation independant of the presence of light. The ease with which the equilibrium of the principles of the phosphyds of ammonia is destroyed, depends on the ease with which light takes its repulsive projectile form. We are in want of a set of accurate experiments on the process of oxydation and phosphydation. It is probable that many metals, besides those we have mentioned, are capable of combining with phosphygen. The different colors and properties of the different oxyds of the same metal may probably depend on certain quantities of light and oxygen entering into their composition.

Such then are the inorganic combinations of light. From the discovery of them we are enabled to explain the phenomena of combustion, detonation, &c. They open to us an

extensive field for experimental investigation. It is probable that we shall detect light in many other substances, in which it has not been hitherto suspected. That extensive class of substances that has been heretofore distinguished by the common name of oxygen-attracting, must be divided into distinct classes, from the differences of the attractions of the substances composing it. Some of these substances, as phosphorus, sulphur, &c. are simple attractors of oxygen, that is, decomposers of phosfoxygen. Others, as iron, zinc, copper, &c. under different circumstances, appear to attract oxygen and portions of light with oxygen. Another class combines with phosfoxygen, or light and oxygen, without decomposition, as nitrogen, muriatic acid, mercury, &c.

We perceive a correspondence between the quantities of light and oxygen entering into the composition of bodies, and their colors. By attending to this circumstance, and by determining the quantities of light liberated in oxydation, and comparing the properties of the oxyds,

'phosoxyds, &c. we may make some discoveries in an important branch of corpuscular philosophy hitherto unknown.—The causes of the differences of the capabilities of bodies for receiving the communicated repulsive motion of light, that is, the causes of the differences of their colors.

RECAPITULATION.

This recapitulation is designed to present the theory founded upon the experiments described, in a short view capable of being at once considered by the mind.

1st. Matter is supposed to be endowed with the active properties of repulsive motion and attraction. By the terms repulsive motion and attraction, we simply mean to express the causes of certain effects which are uniformly and constantly produced. In denying the existence of caloric, we do not assert that there does not exist a number of substances which are totally incognisable by our senses ; but we consider all matter as governed by the same laws, and as active properties must be ascribed to some matter, and as we perceive the effects of them in that matter with which we are acquainted, we have a right to conclude that they belong to it. Nothing is more unphilosophic than to imagine

beings for the sake of attributing to them active powers, when our sensations inform us of the existence of bodies to which they belong.

2. The most subtile etherial fluids with which we are acquainted, are governed by the laws of attraction and repulsive motion. Amongst these is light, which acts the most important part in the œconomy of the universe. This substance is subject to the common laws of matter, and requires no principles, but attraction and repulsive motion, to account for its appearances and changes.

3. It enters into combination with bodies. In the phosphorescent bodies it exists in a state of loose combination.

In phosfoxygen it is intimately combined with oxygen.

4. From the decomposition of phosfoxygen by bodies that attract oxygen, the phænomena of combustion are explained,

5. Phosfoxygen combines with substances without decomposition.

These substances are nitrogen, muriatic acid, and certain metals. On the combination of phosfoxygen with these bodies, the phænomena of detonation &c. depend.

LIGHT enters into the composition of living bodies. To understand these combinations is of infinite importance to man. On the existence of this principle in organic compounds, perception, thought, and happiness, appear to depend. We shall proceed to investigate the theory of respiration.

THEORY of RESPIRATION.

The dependance of life on respiration has always rendered it an important subject of consideration to physiologists; but till the discovery of pneumatic chemistry, no rational theory was advanced. It is foreign to our present design to consider all the different theories that have been formed. The theory of respiration now generally received by physiologists, is that advanced by Godwyn, Girtanner, &c. These philosophers assume oxygen gas to be oxygen combined with caloric; and since it is found by experiment that the oxygen gas made use of in respiration is diminished, and carbonic acid and water formed; it is asserted that oxygen gas is decomposed in the lungs. It is said, that one portion of the oxygen combines with the oxydable and acidifiable bases in the venous blood, and particularly with the iron; and that from this oxydation arises the vermilion color

of the arterial blood : another portion combines with the carbon of the blood and of the pulmonary mucus, to form the carbonic acid liberated in respiration ; another portion combines with the hydrogen of the blood, to form the water liberated in respiration : the caloric combined with the oxygen partly combines with the blood, now increased in capacity, and is partly liberated, with the carbonic acid and aqueous gas. Without considering my experiments, or the theory of these papers, which directly overturn this hypothesis, there are the following objections to it.

1st. Iron which is generally assumed to be the reddening principle of the blood, never decomposes phosfoxygen at so low a temperature as 98° , the greatest heat of the lungs, and phosfoxygen is never decomposed by iron without rapid combustion, flame, and great heat.

2d. Oxygen gas is never decomposed by carbon at so low a temperature as 98° , and is never decomposed without combustion, &c.

3d. There is never a decomposition of phos-oxygen by hydrogen at so low a temperature, and it is well known that this process does not take place without flame.

This theory of respiration, then, is evidently false. It will appear from the following experiments and observations :

1st. That phos-oxygen (light and oxygen) is not decomposed in the lungs.

2d. That phos-oxygen (light and oxygen) combines with the venous blood in the lungs.

3d. That carbonic acid and water are both liberated from the lungs during this process, either by the increase of temperature, or from the superior affinity of phos-oxygen for the venous blood.

EXPERIMENT XVII.

A phial containing twelve cubic inches and a half, was filled with very pure phos-oxygen.

The medial vein of a healthy man was opened, and the stream of blood directed into the phial. The mouth of the phial was immediately brought in contact with the arm, so as to entirely exclude all external air. The room was then darkened. As the blood flowed in, it changed from a dark red to a bright vermilion color. When the phial was half full it was closed, and plunged in mercury heated to 90° . After remaining in this situation for half an hour, it was examined. The blood had coagulated, and was of a bright vermilion; some drops of water were formed on the sides of the phial. When the cork was drawn, about two cubic inches of mercury rushed into the phial, from whence I concluded that an absorption of gas had taken place. The gas remaining in the phial was examined. It proved to be three cubic inches and one tenth of phosphygen, mingled with nine tenths of a cubic inch of carbonic acid.

During this experiment no light was liberated; it is consequently reasonable to suppose that there was no decomposition of phosphygen;

and as a considerable diminution of phosoxygen took place, and the blood acquired new properties, we may conclude that phosoxygen is capable of combination with the venous blood. To prove this by analysis as well as synthesis, I made the following experiment.

EXPERIMENT XVIII.

A phial containing about twelve cubic inches, having a pneumatic apparatus affixed to it, was filled with arterial blood from the carotid artery of a calf. The phial was placed in a sand bath of the temperature of 96° , and the heat gradually and slowly raised. In about ten minutes the temperature of the bath was 108° , and the blood began to coagulate. At this moment some globules of gas were perceived passing through the tube. Gas continued to pass in very small quantities for about half an hour, when the temperature of the sand was about 200° ; the blood had coagulated perfectly and was now almost black: about a cubic inch and eight tenths of gas were collected in the

mercurial apparatus; of this one cubic inch and one tenth was carbonic acid, and the remaining seven tenths phosfoxygen.*

From this experiment it is evident that the arterial blood contains phosfoxygen, and we have proved before by synthesis, that it is capable of combining with it directly. We are possessed, of a number of experiments, which prove that phosfoxygen is consumed in respiration. It has been likewise proved that gases can penetrate through moist membranes like those of which the vessels in the lungs are composed. We may therefore conclude that phosfoxygen combines with the venous blood of the system in the pulmonary vessels. As no light was liberated in Experiment XVII. it is evident

* Great caution is requisite in making this experiment. If the temperature is not gradually and slowly increased, the liberated gases are carbonic acid and hydrogen. The first time that I made this experiment, raising the temperature too quickly, I obtained only these products. At a high temperature, the phosfoxygen of the blood most probably combines with nitrogen, to form nitric phosphacid.

that there cannot be even a partial decomposition of phos oxygen in respiration, and consequently the carbonic acid and aqueous gas liberated cannot arise from the decomposition of phos oxygen by the carbon and hydrogen of the venous blood. It is then evident that they must be liberated from the venous blood. To prove this more clearly, I made the following experiment.

EXPERIMENT XIX.

I filled a small sheep's bladder with blood from the medial vein of an healthy woman. This blood never came in contact with any air during the experiment. The bladder was inserted in a vessel of water heated to 112° , and the gaseous products received by a pneumatic apparatus. They were carbonic acid and aqueous gas.

Respiration, then, is a chemical process, the combination of phos oxygen with the venous blood in the lungs, and the liberation of carbonic acid and aqueous gas from it. From the combination and decomposition, arises an

increase of repulsive motion, which combined with that produced by the other chemical processes taking place in the system, and that generated by the reciprocal action of the solids and fluids, is the cause of animal heat ; a heat which the other systems have supposed to arise chiefly from the decomposition of oxygen gas (oxygen and caloric).

Such then is the human respiration ; and we are certain not only from analogy, but from experiments, that the breathing process of quadrupeds and birds is similar. Phos oxygen (oxygen and light) combines with the venous blood in their lungs.

As fishes exist in a different element ; and as it has been supposed that they decompose both water and oxygen gas,* I endeavoured to ascertain by the following experiments the laws of their respiration.

* Darwin's Zoonomia, Vol I. p. 472. Oxygenation of the Blood.

EXPERIMENT XX.

I expelled by long boiling the atmospheric air from 64 cubic inches of sea water. This by means of mercury I entirely excluded from the contact of air. A small mullet was put into it, which instantly appeared much convulsed, and died in a few minutes.

EXPERIMENT XXI.

A quantity of water was freed from atmospheric air by boiling. Two receivers, each of the capacity of 36 cubic inches, were filled with this water. Into one of these, two cubic inches of nitrogen were inserted, into the other, two of phosphygen. By long and constant agitation, the gases were dissolved by the water, which was excluded from the contact of air by mercury. Into each of the receivers two minnows were inserted. Those in the water holding nitrogen in solution, died in about four minutes ;

those in the water holding phosfogxygen in solution, appeared totally uninjured, and when examined after some hours, were still alive and healthy.

EXPERIMENT XXII.

The same receivers which I used in my last experiment, were filled with distilled water, freed from atmospheric air by a second boiling. Into each of these, three cubic inches of phosfogxygen were inserted. The receivers were then agitated for some time, till the water in each of them had dissolved an equal quantity of gas; they were then inverted in a trough of mercury, so as to exclude atmospheric air from them. Four minnows were then conveyed into one of them through mercury. The receivers were now suffered to remain untouched for six hours, when they were examined. The minnows were alive, and no gas remained in the top of the receiver in which they had respired. The gas in the top of the other receiver remained nearly the same as at the commencement of the experiment.

A quantity of lime-water was poured into each of these receivers ; in that in which the fish had existed there was a very perceptible cloudiness, occasioned as I suppose by the formation of carbonate of lime ; in the other there was no perceptible change.

From these experiments I conclude that the venous blood in the gills of fishes is phosoxydated by the phosxygen held in solution by water ; and that carbonic acid, and probably water, are given out as excrementitious by the venous blood in their gills. We have no reasons for supposing that fish decompose water, as we cannot discover that any hydrogen is formed by them in respiration.*

Light and oxygen then, (phosxygen) are essential to life.

* I have discovered by similar experiments that the zoophyta are governed by similar laws : that they, like fish, absorb the phosxygen held in solution by water, as well as portions of nitrogen ; and thus in their chemical attractions, as well as in their organic powers, seem to be the connecting links between vegetables and animals.

The perceptive and volitive powers depend for their continued existence on the constant supply of a certain quantity of phosoxydated blood to the nervous and muscular systems.

Perception more immediately depends on the continued supply of a certain quantity of arterial blood to the brain. In the brain and nervous system, some important change essential to life must be effected by it. As there is a necessity for a constant supply of phosxygen to support the vital functions, there must be a constant expenditure of it in the performance of these functions. The medullary substance of the brain and nerves appears to possess the property of sensibility. This property is perfectly distinct from the irritability or contractile power of the muscular fibre. The nerves depend for their sensibility on their connection with the brain. In the brain all the sensations conveyed by the different nerves center; and in the brain their correspondent ideas are associated together according to certain laws. The moment that the connection of a nerve with the

brain is destroyed, it ceases to be sensible ; and thought and action cease the moment that a supply of phosoxydated blood is cut off from the brain.

May we not venture to reason on the important and constant change effected in the brain and nerves by the phosoxydated blood ? Is it not probable that the existence of some fine ethereal principle in the brain and nerves, is the immediate cause of sensible or perceptive action ? If such a fluid exists, it must be continually supplied by the arterial blood, and constantly expended in sensible action. We have proved the existence of light in the arterial blood, and we have likewise proved its existence in different states. Is it then improbable to suppose that LIGHT is attracted or secreted from the blood by the brain in the form of an ethereal fluid or gas, and perpetually conveyed by the brain to the nerves ?

A number of philosophers simply from the identity of the action of the electric fluid, and

the nervous influence on the irritable fibre, have concluded them to be the same, that is, have concluded the nervous fluid to be the electric aura.

We have before supposed the electric fluid to be condensed light. Thus we have another cogent reason for supposing that the nervous spirit is light in an ethereal gaseous form.

On this supposition, sensations and ideas will be motions of the nervous ether or light exciting the medullary substance of the nerves and brain into sensitive action. The capability of the nerves to be excited into sensitive action by the motions of the nervous ether, must depend upon a peculiar constitution or organization of them; and the nerves during sensitive action must suffer some change, some loss of principles, and the equilibrium of their principles must be again supplied by the arterial blood.

The irritability of the muscles, as well as the sensibility of the nerves, appears to depend on

the continued supply of a certain quantity of arterial blood. Their irritability is not nearly so soon destroyed as the sensibility of the nerves. The fibre remains irritable for a considerable time after it is deprived of arterial blood. The muscles are most probably phosphydated compounds, of which the numerous principles are in exact and delicate equilibrium; and it is likely that on this equilibrium their irritability depends. Any communicated impulse capable of producing increase of repulsive motion sufficient to occasion a new arrangement of principles in any part of the fibre, will produce irritable action. The impulses of certain external bodies, and the nervous motions, both appear equally capable of exciting the irritable fibre into action.

The chief principles* of the fibre appear to be nitrogen, hydrogen, carbon, oxygen, and

* The inorganic compounds, which contain light, and many other principles, are most easily decomposed by the slightest increase of repulsive motion, such are the phosphyds of ammonia.

light.† The immediate cause of irritable action is probably the combination of the oxygen with the hydrogen and carbon, to form water and carbonic acid, and the liberation of azote and electric fluid. We are certain that water and carbonic acid are liberated during muscular action, and probably azote, and light in the form of electric fluid.*

† When any considerable change takes place in the organic matter of the body, so as to destroy the powers of life, new chemical attractions and repulsive motions take place. The different principles of which the body is composed, form new combinations. In this process, which is called putrefaction, the light of the system in land animals in combination with oxygen and nitrogen, forms nitric phosphacid.

* In fish, during whose putrefaction no nitric phosphacid is formed, it is liberated; and hence the reason for the luminous appearance of putrefying fish, an appearance, which Lavoisier supposed to be occasioned by phosphorised hydrogen. I have found by experiment, that putrefying fish are equally luminous in water boiled to expel its air and phosphoxygen.

* The Torpedo, and some other animals, give out electric fluid during animal action. In man the quantity is probably however too small and too slowly liberated to be ascertainable. It would be worth while to try, by a very sensible electrometer, whether an insulated muscle when

Life, then, may be considered as a perpetual series of peculiar corpuscular changes; and the living body as the being in which these changes take place. Perceptions, ideas, pleasures, and pains, are the effects of these changes. They are consequently found to be continually varying. The laws of mind then probably, are not different from the laws of corpuscular motion. Every change in our sensations must be accompanied with some correspondent change in the organic matter of the body. These changes an extensive and philosophic chemistry may enable us to estimate.

Thus essential then is LIGHT to perceptive existence. All organic sensitive beings with

stimulated into action, would not give marks of the liberation of electric fluid.—To ascertain the gaseous products liberated during muscular action, would not be so difficult. An animal or the limb of an animal, might be stimulated into muscular action for a considerable time under mercury, till its irritability was destroyed. Animals previously made to breathe phosphygen, might be employed for this purpose, as it appears from the experiments of a celebrated philosopher, Dr. Beddoes, that they retain their irritability longer.

which we are acquainted appear totally unable to exist without phosphygen.

We may consider the sun and the fixed stars, the suns of other worlds, as immense reservoirs of light destined by the great ORGANISER to diffuse over the universe organisation and animation. And thus will the laws of gravitation, as well as the chemical laws, be considered as subservient to one grand end, PERCEPTION. Reasoning thus, it will not appear impossible that one law alone may govern and act upon matter : an energy of mutation, impressed by the will of the Deity, a law which might be called the law of animation, tending to produce the greatest possible sum of perception, the greatest possible sum of happiness.*

* The analogy between attraction and gravitation, repulsion and projection, has been mentioned before. This analogy would induce us to refer them to the same causes. It may appear absurd to suppose any analogy between these powers and the laws of life ; Is not however perceptive

The farther we investigate the phænomena of nature, the more we discover simplicity and unity of design.

An extensive field for sublime investigation is open to us. The laws of perceptive life as yet are but partially known. Our sensations, ideas, pleasures, and pains, depend upon causes now unknown to us.

We cannot entertain a doubt but that every change in our sensations and ideas must be accompanied with some correspondent change in the organic matter of the body. These changes experimental investigation may enable us to determine. By discovering them we should be informed of the laws of our existence,

action (which must uniformly be accompanied with some peculiar motion in the nervous system) analogous to repulsion and projection? Is not the association of perceptive and irritative motions, a law analogous to attraction and gravitation?

and probably enabled in a great measure to destroy our pains and to increase our pleasures.

Thus would chemistry, in its connection with the laws of life, become the most sublime and important of all sciences.

An ESSAY
On the GENERATION of PHOSOXYGEN,
Or OXYGEN GAS;
And on the Causes of the Colors of
ORGANIC BEINGS.

An E S S A Y
On the GENERATION of PHOSOXYGEN,
(OXYGEN GAS)
And on the Causes of the Colors of
ORGANIC BEINGS.

A great quantity of phosxygen is continually attracted from the atmosphere to supply the pabulum vitæ of perceptive beings. Another considerable quantity is decomposed by combustion, and absorbed in other chemical processes which take place in our globe. Since the atmospheric constitution is uniformly similar, we are led to enquire by what means a quantity of phosxygen equal to that consumed by respiration and combustion is again supplied to the atmosphere. This enquiry is no less important than curious, as our existence itself

depends on the equilibrium of gases in the atmosphere.

We have no reason for supposing that phos-oxygen is generated or given out as excrementious by the locomotive organic beings which consume such immense quantities of it. During living action, water, carbonic acid, and nitrogen, are liberated from the animal, and probably electric ether and some other products.

We have no reasons for supposing that light is capable of decomposing either water or carbonic acid in their nascent state, by its attraction for oxygen. Consequently the phos-oxygen of the atmosphere can in no way be supplied by the locomotive perceptive beings.

The living action of another class of beings, namely vegetables, is the cause of the generation of the atmospheric phos-oxygen.

The purification of the atmosphere by land vegetables was indeed discovered long before

even its composition was known. We owe this important discovery to the immortal Priestley, who supposed that the renovation of the atmosphere by vegetables was occasioned by their power of absorbing phlogiston, an imagined product of combustion and respiration.

Dr. Ingenhouz discovered that vegetables gave out vital air when exposed to the solar light in contact with water. This philosopher attempted to prove by experiments, that the air thus generated arose from the decomposition of water. Since his experiments and those of M. Senebier have been published, we know of little that has been done in this part of chemistry : and as Lavoisier's theory of the composition of oxygen gas (phos oxygen) has been generally believed by chemical philosophers, it has been supposed that light acted no other part in the decomposition of water, than that of extricating oxygen by giving it caloric, whilst the vegetable attracted hydrogen.

In my essay on Repulsive motion and Light,

I attempted to prove that water was decomposable by two attractions, that of a vegetable or organised hydrogen attractor for hydrogen, and that of light for oxygen. In that essay I likewise mentioned my discovery of the production of phosphygen by the various orders of the marine cryptogamia class of plants. I shall proceed to give a detail of the experiments I have made on land and sea vegetables.

One might infer from the analysis of vegetables that they attract hydrogen and carbon, as they are their most predominant principles. It will appear from the following synthetical experiment that they attract hydrogen.

EXPERIMENT I.

A small plant of *Minianet* in a state of healthy vegetation was inserted in a small pot under a jar of hydrogen containing 28 cubic inches, and confined by mercury. The height of the mercury in the jar was accurately marked at the commencement of the experiment. It was

suffered to remain in the dark for fifty hours, and then examined. The mercury had ascended a very little, and if one might judge from appearances, there was a diminution of about half a cubic inch. The jar was removed to a place exposed to the light, and suffered to remain in this situation for three days of very fine weather. It was then examined: the plant appeared uninjured, and the edges of some of its leaves were tinged yellow; there was no perceptible diminution of the mercury, from whence I concluded that phosphygen had been generated. Solutions of sulphure of potash, and of potash were introduced into the jar in separate vessels. A diminution took place. The remaining gas measured about twenty five and half cubic inches, and appeared to be pure hydrogen.

Since, then, two cubic inches and half of hydrogen disappeared in this experiment, they were most probably absorbed by the plant.

I have found by several experiments, that *Minianet* always grows very well in hydrogen when supplied with water. One of these plants lived in hydrogen for a week in the dark, supplied with water by a capillary tube. At the end of this time the leaves became variegated with yellow spots, and the whole of the plant bore a sickly appearance. When these plants growing in hydrogen are deprived of water, they generally die in three or four days. In the experiment that I have just related, the earth of the pot in which the plant grew was moistened previous to its insertion, and then covered with a tin-plate top, to prevent the water from evaporating. Some plants die in hydrogen very quickly; among these are *Conium maculatum*, *Chironea centaurium*, *Digitalis purpurea*.

In a mixture of hydrogen and carbonic acid, I found no plant to die when exposed to light. From a number of experiments, I think I have every reason to conclude that plants directly combine with hydrogen; but as they owe their

irritability, that is, their life, not to one principle alone, but to many, it is difficult to invent simple experiments to determine singly their attraction for each.

As vegetables attract hydrogen, and as it is one of their most predominant principles if it be considered as a simple substance, it must be obtained by them from the decomposition of bodies into which it enters as a constituent part.

Vegetables are totally incapable of decomposing water by their attraction for hydrogen. Some land plants give out carbonic acid, and small portions of nitrogen during the night ; but there is no single instance of the production of phosxygen from plants during the night. To determine whether the water plants which strongly attract hydrogen produce any gas in the absence of light, I made the following experiment.

EXPERIMENT II.

Into a green glass globe containing one hundred and four cubic inches, a plant of *nymphaea alba* in a state of healthy vegetation was inserted. The globe was filled with water previously boiled to expel the atmospheric air, and inserted horizontally in a vessel, so as to be perfectly included from light. After remaining in the dark for four days it was examined. Two or three inconsiderable globules of gas were found in the top; which suffered no diminution when mingled with nitric phosphyd (nitrous gas). The vegetable was pale and sickly, and water had most probably combined with it.

Water then is not decomposable by the attraction of vegetables for hydrogen, nor is it capable of decomposition by the attraction of light for oxygen.

The experiments of Priestley, Ingenhouz, and Senebier, prove that land vegetables of all kinds produce greater or less quantities of phosphyd, when exposed to light in contact with

water. I have repeated some of the experiments of Ingenhouz, and have obtained nearly the same results as that philosopher. I endeavoured to establish the fact of the decomposition of water by light and an organised hydrogen attractor by the following experiment.

EXPERIMENT III.

A glass cylinder of the capacity of ten cubic inches was filled with mercury. Two small vine leaves were introduced through the mercury, so as to detach all atmospheric air from them. The mercurial apparatus was now inserted in a vessel of cold water. Aqueous gas was passed from a vessel containing water which had been long in ebullition, through a long tube into the cylinder, where it was condensed by the cold mercury. In this manner the cylinder was filled with water which held no air in solution.

The cylinder still inverted in the mercurial trough, was now exposed to light. In a very short time air globules began to form on the

leaves, and in about six hours sufficient was formed to be examined. It measured two cubic inches and half, and was nearly pure phos oxygen.

Since in this experiment no gas of any kind was held in solution by the water, and pure phos oxygen was produced from it, it must have arisen from the decomposition of the water, by the combination of its oxygen with light, and of its hydrogen with the vegetable.

An immense quantity of water then must be constantly decomposed by the attractions of land vegetables and of light ; and a considerable quantity of phos oxygen must be continually supplied to the atmosphere from this source.

Carbonic acid, as well as water, is formed in large quantities by combustion, fermentation, &c. and is continually liberated during the living action of perceptive beings. Now as this gas is considerably heavier than atmospheric air, if it was not perpetually decomposed by

some means, the lower strata of the atmosphere would soon become composed of it, the supply of phosxygen would be cut off from locomotive organic beings, and perception, thought, and action, would cease to exist. As carbon forms one of the most predominant principles of land vegetables, we might conclude almost without experiment, that the carbonic acid of the atmosphere is somehow decomposed by them. To determine whether land vegetables were capable of decomposing carbonic acid by their attraction for carbon, unassisted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT IV.

A small plant of *chironca centaurium* growing in a pot of very dry earth was inserted under mercury, in a jar filled with very pure carbonic acid. It remained in a dark closet for four days, and was then examined. The mercury in the jar had ascended a very little, and the plant had a pale and sickly appearance. After the carbonic acid was taken up by potash,

there remained in the jar a very small portion of an incombustible gas.

From this experiment it is evident that the attraction of plants for carbon is too weak to enable them to decompose carbonic acid ; but that they combine with small portions of it, and thus supersaturated with oxygen become white and sickly.

Light alone effects no decomposition of carbonic acid, not even in its concentrated state, in the electric spark.

To determine whether plants were capable of decomposing carbonic acid, assisted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT V.

I planted in each of two pots filled with moist earth, a small *arenaria tenuifolia*. The two plants were of equal weights, and nearly

fimilar. They were inferted into two jars containing each fourteen cubic inches, one of which was filled with nitrogen, and the other with carbonic acid. The jars were inverted in mercury, and expofed to the folar light for four fucceffive days of fine weather in the month of July. The plant in the carbonic acid now looked well and healthy, that in the nitrogen dark and faded. The mercury in the jar filled with carbonic acid had afcended confiderably. The gafes were now examined. In the jar with the carbonic acid there was a deficiency of 2,3 cubic inches. After the carbonic acid was taken up by potafh, the remaining gas meafured two cubic inches, and proved to be phofoxygen. In the jar containing nitrogen, the gas was not diminifhed more than three tenths of a cubic inch. On the introduction of fuphure of potafh a fmall diminution took place, amounting to fix tenths of a cubic inch. This diminution was, I fuppose, owing to the abforbtion of phofoxygen formed from the decomposition of the water of the plant.

From this experiment, which I have repeated two or three times on different plants with similar results, I conclude that carbonic acid is decomposable by the force of two attractions; that of light for oxygen, and that of certain organised carbon attractors for carbon.

Thus then we find in the vegetable world a source for the decomposition of the carbonic acid and water formed in respiration and combustion. It is chiefly by the means of vegetables that the equilibrium of the atmosphere is preserved; and on their chemical influence, and that of light, do we depend for our existence.

Animals may be considered as absorbing in their respiration phosphygen, and as taking in in their nutriment, hydrogen, carbon, nitrogen, and oxygen, and giving out perpetually these principles in new combinations; water, carbonic acid, and probably ammonia and electric fluid. On this perpetual series of changes their life appears to depend. Vegetables, on the other hand, are continually absorbing water, carbonic acid, and probably ammonia and nitrogen, and

affisted by light in the exercise of their organic functions giving out phosfoxygen.

Without vegetables animals would cease to exist, and were locomotive perceptive beings removed from the earth, vegetable life would soon be at an end.

The vegetation of land plants then may be considered as the great source of the renovation of the atmosphere of land animals.

We shall proceed to investigate the manner in which the inhabitants of the ocean are supplied with phosfoxygen. For as the experiments in my last essay prove, they continually require it as well as land animals.

I have found by experiments, that water equally dissolves phosfoxygen and nitrogen, and that nitrogen is not expelled from water by phosfoxygen. The myriads of inhabitants that people the immensity of the ocean are continually absorbing phosfoxygen, and giving out carbonic acid, &c.

The sea water may be considered as continually dissolving atmospheric air. Now if there were no source for the absorption of the nitrogen and carbonic acid held in solution by water, and for the production of a quantity of phosfoxygen equal to that absorbed by fish, the waters of the ocean would soon become saturated with nitrogen and carbonic acid, and the perceptive inhabitants of it would cease to exist.

As land vegetables are the renovators of the atmosphere of land animals, analogy led me to suppose that sea vegetables must be the preservers of the equilibrium of the atmosphere of the ocean.

I first attempted to determine whether like land vegetables, they produced phosfoxygen during the presence of light ; for this purpose I made the following experiment.

EXPERIMENT VI.

Into a green glass globe containing a hundred and eighty four cubic inches of sea water,

I inferted a large sprig of fucus natans, another of conferva fœniculacea, and three or four pieces of an ulva with the name of which I am unacquainted. The globe was exposed to the solar light. It had not been exposed many minutes, when I beheld with great pleasure, globules of gas forming on the vegetables, which in a short time were raised by them to the top of the globe. In about a quarter of an hour, a large globule was formed, and in about two hours, sufficient to measure. It was one cubic inch and half, and by trial with nitric phosoxyd, proved to be $\frac{59}{100}$ parts phosoxygen, and $\frac{41}{100}$ azote.* During this experiment, the thermometer stood at 47°, and the barometer at 29,9.

Having ascertained that a compound gas was produced from water by the marine cryptogamiæ, with a larger proportion of phosoxygen than atmospheric air, I made the following

* A gas indiminithable by nitric phosoxyd.

experiment to ascertain whether all the marine cryptogamia produced this gas, and whether it was purer when the dissolved air was expelled from the water by boiling.

EXPERIMENT VII.

Into a green glass globe containing a hundred and four cubic inches, filled with sea water previously well boiled to expel the atmospheric air, I inserted two cubic inches of fucus fibrosus. The globe was inserted in a vessel of boiled water, and exposed to a bright sunshine. Soon after its exposure the fucus became covered with a vast number of globules of air, which raised it to the top of the globe, and then detached themselves to form larger globules. In about four hours sufficient gas was formed to ascertain the quantity and quality of it. It measured two cubic inches and a quarter. The mercury in the barometer standing at 30 inches, and the thermometer at 54°. By trial with azotic phosphyd it proved to be $\frac{71}{100}$ phosphygen, and $\frac{29}{100}$ azote.

The globe being exposed the next day for six hours in a very bright sunshine, three cubic inches were formed; about $\frac{76}{100}$ phosfoxygen, and $\frac{24}{100}$ azote.

The day after, the globe was exposed for nine hours and half of moderate weather; three cubic inches and half were formed, of nearly the same quality as the last.

On the fourth day of this experiment the sun shone very bright for five hours; two cubic inches were formed, $\frac{18}{100}$ azote, and $\frac{82}{100}$ phosfoxygen.

All the gas in this experiment which was not diminished by azotic phosfoxyd, was put into a vessel, and when examined, proved to be two thirds nitrogen, and one third carbonic acid.

From this experiment it appears that the fuci produce phosfoxygen during the presence of the solar light. The nitrogen produced in this experiment most probably arose from a

finall portion of atmospheric air, which water still holds in solution, though boiled.

EXPERIMENT VIII.

Two cubic inches of *ulva dichotoma* were inserted into the globe containing a hundred and four cubic inches, filled with water previously boiled. It was inserted in a jar containing water of a similar kind. This globe was exposed for four days successively to the solar light. On the first day, in eight hours of sunshine, three cubic inches and half of gas were formed, $\frac{70}{100}$ phofoxygen, and $\frac{30}{100}$ azote.

On the second day, when the sunshine was uncommonly bright, in six hours two cubic inches and half were formed, of $\frac{77}{100}$ phofoxygen, and $\frac{23}{100}$ azote.

The third day was showry, and the sun often obscured; in nine hours but two cubic inches and quarter were formed, containing $\frac{76}{100}$ phofoxygen.

The sun was out and very bright continually for seven hours on the fourth day; two cubic inches and half were formed, $\frac{83}{100}$ phosfoxigen, and $\frac{17}{100}$ azote.

EXPERIMENT IX.

Into the green glass globe of a hundred and four cubic inches, filled with boiled sea water, two cubic inches of *conserva littoralis* were inserted, and the same precautions being taken as in the last experiment, the globe was exposed to the solar light for four hours; in this time five cubic inches of gas were formed, of the quality of 76.*

The next day the globe was exposed for seven hours of sunshine, and six cubic inches were formed, of the quality of 78.

* To prevent unnecessary repetitions, I shall for the future express the quality of gases by giving the quantity of phosfoxigen in 100 parts, from whence the quantity of azote will be known.

The day after, the globe was again exposed for eight hours ; five cubic inches were formed, of the quality of 81.

On the fourth day, the weather in the morning was cloudy ; but in the afternoon the sky brightening, it was exposed for three hours and half, and in this time two cubic inches were formed, about 86.

The gas produced in this experiment, unalterable by nitric phosphyd, was chiefly nitrogen.

During the whole course of these experiments I perceived no sensible difference in the production of gas corresponding to difference of temperature.

From these experiments it is evident that the different orders of the marine cryptogamia are capable of decomposing water assisted by the attraction of light for oxygen.

I discovered by analysing the sea-weeds, that they were composed chiefly of hydrogen, carbon, and nitrogen.

As carbon is incapable of solution in water, it is evident that the sea-weeds must obtain it from the decomposition of some of its combinations. I found that sea-weeds placed in water saturated with carbonic acid, produced no alteration in it in the dark for some days, except that of disengaging portions of the dissolved carbonic acid.

To determine their capability of decomposing it assisted by the attraction of light for oxygen, I made the following experiment.

EXPERIMENT X.

Into a vessel containing sixteen cubic inches of carbonic acid, two cubic inches of *conferva scopularis* were inserted. The vessel was inverted in mercury, and exposed to the solar light. It remained in a bright sunshine for eight hours, and at the end of that time was examined. The gas was diminished to eleven and half cubic inches, so that there was a deficiency of five cubic inches and half. Of the eleven and half cubic inches remaining, two

were phosfoxygen, the remainder pure carbonic acid.

To determine that the whole of the phosfoxygen produced in this experiment did not arise from the decomposition of the water in the vessels of the plant, I made the following comparative experiment.

EXPERIMENT XI.

Two vessels, containing each ten cubic inches, were filled, one with carbonic acid, the other with nitrogen, under mercury. Into each of these, sixty grains of *conserva littoralis*, previously wiped dry, were inserted. The vessels were exposed to the solar light for six hours, and then examined. The carbonic acid was diminished two cubic inches, the nitrogen not more than one fourth of an inch. One cubic inch and seven tenths of phosfoxygen was found mingled with the carbonic acid, with the nitrogen only six tenths of phosfoxygen.

EXPERIMENT XII.

Two glass globes, containing a hundred and four cubic inches each, were filled, one with boiled sea water, and the other with sea water holding in solution carbonic acid. Into each of them was inserted a hundred grains of fucus natans. They were then exposed to the solar light for four hours, and at the end of that time examined. In the vessel containing carbonic acid and water, three cubic inches of gas were formed, sixty-four parts phosphygen, sixteen carbonic acid, and twenty nitrogen. In the vessel with water, one cubic inch and six tenths, of the quality of 75.

From these experiments it is evident that carbonic acid is decomposable by the attraction of the marine cryptogamia for carbon, and that of light for oxygen.

Since nitrogen is one of the principles of the marine cryptogamia, and since it is held in solution by water in a very large proportion in

atmospheric air, it occurred to me that the marine cryptogamia attract it from the atmospheric air dissolved in the sea, and combine with it. To ascertain this I made the following experiment.

EXPERIMENT XIII.

A glass jar containing thirteen cubic inches, was filled with nitrogen under mercury. Into this, two cubic inches of *conserva purpurascens* were inserted, and the quantity of gas in the jar accurately determined. Thus disposed, the whole apparatus was suffered to remain in the dark ; the mercury in the thermometer standing at 61°. After four days the apparatus was examined ; the mercury had ascended considerably, and the gas was measured. There was a deficiency of a cubic inch and half. I could not discover that any gas had been given out by the plant.

EXPERIMENT XIV.

A small glass vessel containing eight cubic inches, was filled with nitrogen under mercury.

Into this two cubic inches of *fucus vesiculofus* were inserted. The apparatus was exposed to the solar light. After remaining for two days of fine weather, the gas was examined; there was a deficiency of a cubic inch and quarter, and half an inch of the gas remaining was phos-oxygen. A cubic inch and three quarters of nitrogen were consequently absorbed by the plant.

The similarity of the constitution of the atmosphere depends on the equilibrium between the gases absorbed, and those generated.

Locomotive perceptive beings are continually absorbing phos-oxygen in their respiration, and giving out carbonic acid, water, &c. Vegetables, assisted by the attraction of light for oxygen, are continually decomposing water and carbonic acid, of which the oxygen is liberated in combination with light, and the hydrogen and carbon combine with them.

Nitrogen is probably liberated during animal action ; and this principle is likewise absorbed by some of the vegetables.

The equilibrium of the gases held in solution by the waters of the ocean is preserved by the marine cryptogamia class of vegetables.

The marine animals are continually absorbing the phosxygen of the atmospheric air held in solution by water, and giving out carbonic acid, and probably other principles.

The marine vegetables are continually absorbing the nitrogen of the atmospheric air held in solution by water; and assisted by the attraction of light for oxygen, decomposing water and carbonic acid, combining with the hydrogen and carbon, and liberating the oxygen in combination with its attractor light.

Thus the sea-weeds that every where cover the rocks at the bottom of the ocean, are continually giving out phosxygen during the pre-

fence of light. In the deeper parts of the ocean the phoxygen, continually formed in small globules, is probably almost wholly dissolved by the sea-water before it can reach the atmosphere above ; but near the sea-coasts where the marine conservæ are found in immense quantities, the greater portion of the phoxygen formed on their leaves is liberated into the atmosphere.*

From this discovery we are enabled to account for a number of phænomena before inexplicable.

1st. In what manner the equilibrium of gases dissolved in the ocean is maintained.

* The following is an account of the composition of the atmosphere in different parts of Mountsby in Cornwall, where the experiments on the marine cryptogamia were made. On a calm morning in May 1798, the mercury in the barometer standing at 29,3 and that in the thermometer at 55, the air in the middle of Penzance contained 28,5 per cent. phoxygen. The air on the rocks of the sea-shore 30. The air on the sea about a furlong from the shore, nearly the same proportion ; at a quarter of a mile from the shore, the proportion was 30,12. About a mile, 30,1. At two miles 29,12, and at four miles 29. On a calm morning the air at the land's end was 29,13.

2d. Why the sea air is purer than that of the land.

3d. Why the air near the sea coasts is even purer than that on the ocean.

Nature then has catenated together organic beings, and made them mutually dependant on each other for their existence, and all dependant on light.

A privation of light would be immediately destructive to organic existence; vegetation would cease; the supply of phosoxxygen would be quickly cut off from animals; the lower strata of the atmosphere would become composed of carbonic acid, and perception and volition would exist no longer.

The irritability of the living fibre of plants appears to depend on the equilibrium between its principles. This equilibrium is preserved by light. The principles of the fibre are chiefly carbon, hydrogen, oxygen, and light. Some of

them contain portions of nitrogen. The irritable vegetable fibre appears to be a phosphyd similar in some respects to the animal fibre, but containing smaller proportions of nitrogen, and light. Different vegetables possess different degrees of irritability. The *mimosa sensitiva*, the *dionea muscipula*, and some other plants, possess an irritability so exquisite as to border on the sensibility* of animals. The irritability of the sea-weed is so indistinct as to be scarcely perceptible. The more perfect land vegetables, possessed of a vascular system, require a supply of food slowly and regularly. They possess a power of giving out as excrementious, the principles which are noxious to their existence, and by a beautiful œconomy of nature are capable of reproducing their species.

The marine cryptogamia, in their organiza-

* The observations of a most ingenious philosopher, Dr. Darwin, render it probable that some of the more perfect vegetables possess sensibility. See Darwin's *Zoonomia*, vol. 1, p. 101, and 102.

tion and functions, are very much inferior. Attached to the rocks on the bottom of the ocean, they assimilate to them nitrogen and water, and assisted by the attraction of light for oxygen, combine with hydrogen and carbon; they appear to possess no regular vascular system for the circulation of fluids, and absolutely in their generation and growth appear to follow the law of assimilation of particles, similar to that by which minerals are crystalized.

The light entering into the composition of vegetables appears not only to be the principle on which their irritability more peculiarly depends, but likewise to be the principle to which they as well as all substances chiefly owe the differences of their colors. We do not assert that light is the general coloring principle; this would be absurd in the present state of our knowledge. We shall attempt to demonstrate that many vivid colors depend on combined light.

Almost all the simple substances and the

combinations of the simple substances that contain no light, are either pellucid, white, or black. Among these are the gases, water, alcohol, the acids, the alkalies, the pure earths, phosphorus, sulphur, when in its pure state and uncombined with light,* carbon, and the different metallic substances which in general are either white, or black and white, that is, grey. Copper, which is yellow, appears to contain light, and probably some other metals.

The compounds into which oxygen enters, and which contain no light, as oxyds, acids, &c. are likewise generally either transparent, white, or black; transparent, as the simple acids and some of the oxyds, white, as the oxyds of lead, zinc, and antimony, black, as the oxyd of iron.

The inorganic compounds containing phos-

* Sulphur, in its common state, evidently contains a portion of light, as is evident from the experiment mentioned in that part of the last essay which relates to phosphorescent bodies.

oxygen, are almost all of vivid colors; and these colors appear to be in some measure correspondent to the quantities of light entering into their composition. A small quantity of light combined with oxygen and metallic bodies, renders them either brown, as the argentic and ferric phosoxys,* or purple, as the auric phosoxyd. Bodies containing a larger proportion are either green, as the cupric phosoxyd, or yellow, as the tungstic phosoxyd. The phosoxys containing a still larger portion of light, are bright red, as the plumbic and mercuric phosoxys. The chromic phosacid, as was observed in the last essay, likewise owes its color to combined light.

* In my last essay, treating of the phosoxys, I have neither mentioned the ferric, cupric, or blue cobaltic phosoxys, because I am possessed of no facts which absolutely prove the presence of light in these bodies. This however is extremely probable, not only from analogy, but from the phenomena observed in their oxydation. The variety of colors of the oxys of iron combined with earths &c. depend most probably on the different quantities of light entering into their composition.

The phosphyds lose their colors when light is subtracted from them in combination with oxygen.

When the tungstic phosphyd is exposed to light, it gives out phosphogen, and turns from yellow to blue.

The mercuric phosphyd, on the subtraction of a portion of its light in combination with oxygen, becomes brown.

The plumbic phosphyd likewise becomes brown on a subtraction of a portion of its light and oxygen; when the whole of its light is subtracted, it becomes white.

The sulphuric acid combines with the plumbic oxyd, but not with the plumbic phosphyd. When it is poured on the last, phosphogen is given out, the phosphyd gradually loses its color with its light and oxygen, becomes white, is converted into an oxyd, and combines with the acid.

The green prussiate of iron, on a subtraction of oxygen and light from it, becomes blue.

The whiteness of etiolated vegetables is occasioned by the deficiency of light ; the different shades of green in the leaves of vegetables depend on the light entering into their composition ; and the fine colors of the different flowers appear to be produced by combined light, as will appear from the following experiments and observations.

EXPERIMENT XV.

Two lettuces of equal size, both fine, healthy, green plants, were planted in two pots filled with moist earth ; one of these plants was inserted in a jar filled with carbonic acid, and deprived of light. The other was exposed to light and atmospheric air. In about twelve hours the inferior leaves of the plant in the dark began to fade, which obliged me to remove it out of the jar filled with carbonic acid,

and expose it to atmospheric air. It remained deprived of light for six days, during which time it was plentifully supplied with water. At the end of this time the leaves were very pale, the lower ones perfectly white; on the upper ones a few spots of green remained. It was again deprived of light, and daily supplied with water. After remaining for a week it was again examined: the leaves were now quite white.

The plant which had been exposed to light and air was now examined. It was of a fine lively green, and much larger than the etiolated plant. Equal weights of the leaves were analysed by destructive distillation. The only perceptible difference in the products was a larger proportion of carbonic acid and water in the white, and in the green more hydrogen and residual carbon. I obtained from some of the remaining green leaves, by a low heat, a small quantity of phosphygen mingled with carbonic acid; from the white, carbonic acid alone.

I have often obtained from the green leaves of vegetables, by applying a low heat very gradually, small portions of phosoxygen; from which it appears that the green colors of vegetables depend on the light, or the phosoxygen entering into their composition, and the whiteness of etiolated plants, to the deficiency of light in their composition.

Plants, in the process of etiolation, lose the light combined with their leaves, and become white. They are as well capable of combining with a larger proportion of light, and thus supersaturated with light, become bright colored red or orange, as will appear from the following experiment.

EXPERIMENT XVI.

I procured two lettuces and two plants of sorrel (*rumex acetosa*) of nearly the same size. One of each kind was planted in moist clay, which had been before proved to be fit for vegetation, the other in silicious sand mingled

with a little clay. The plants in the clay were placed under a shady wall and daily watered, those in the sand were placed in a situation constantly exposed to the sun, and were supplied with very small quantities of water. In about six days the plants exposed to light became spotted with red and orange in many parts of their leaves. These spots continued to increase in number and size, and in about a fortnight the color of the upper parts of the leaves of the lettuce were changed to a dark red. The upper parts of the leaves of the *rumex acetosa* were red, and the lower of a dusky green. The plants in the shade were of a fine green. Comparative analyses of equal parts of the green and the red leaves were made. The results of three experiments were, that the red leaves contained more phosphygen, hydrogen, and residual carbon, and the green more carbonic acid and water.

From this experiment it is evident that the red or brown colors of leaves too much exposed to light, is owing to their containing a super-

abundance of light, which in the analysis combined with oxygen.

The different colors of the leaves of different plants then, most probably depend on the different quantities of light, oxygen, and carbon, entering into their composition.

Flowers, like leaves, depend for their vivid colors on the light entering into their composition. The red flowers, as the rose, anemone, &c. appear to contain the greatest quantities of light. I have found by experiment that red rose trees, when carefully included from light before their flowers begin to appear, and supplied plentifully with water and carbonic acid, produce flowers almost white.

By making comparative analysis of the white and red roses, from several experiments, I think I have every reason to conclude that the red roses owe their colors to combined light.

By distilling muriatic acid from red rose

leaves, I discovered that a small portion of the acid was converted into muriatic phosacid, which proves that the red rose leaves contain light and oxygen.

The pink, orange colored, and yellow flowers appear to contain smaller proportions of light; the dark purple and blue still less. In the white flowers I have never been able to detect the presence of light. The fine colors of fruits, which depend entirely on their exposure to light, most probably depend, like those of flowers, on the light entering into their composition.

I have made some experiments on the marine cryptogamia, which seem to prove that they are governed by the same laws of color as land vegetables. I have succeeded in blanching the dark colored conservæ by secluded them from light, and their different colors appear to depend on the different quantities of light, oxygen, and carbon, entering into their composition. Zoophyta and fish, the perceptive inhabitants

of the ocean, appear to depend on similar causes for their colors. I have observed that the zoophyta exposed to light are uniformly brighter colored than those which have been by any means secluded from it, and I succeeded in altering the colors of two sea anemones from a dark red to a pale pink, by secluded them from light.

The parts of fish which are exposed to light, as the back fins, &c. are uniformly colored red, brown, green, yellow, blue, &c. in different fishes ; but the belly, which is deprived of light, is uniformly found white in all of them.

Rational analogy alone would induce us to suppose that the colors of land animals depend upon the same causes. Independent of this, we have facts and experiments sufficient to establish this truth.

The birds that inhabit the tropical countries are much brighter colored than those of the north. Those parts of birds which are not

exposed to light are uniformly pale. The feathers on the bellies of birds are generally pale or white: the back, which is exposed to light, is almost always colored: the breast, which is partially exposed in most birds, is brighter than the belly, and paler than the back. Those parts of the same feather which are exposed to light, are uniformly bright colored, when compared with those parts that are deprived of its influence.

The colors of quadrupids depend on similar circumstances. The beasts of the equatorial countries are uniformly brighter colored than those of the polar countries. The hair on the parts of beasts not exposed to light is uniformly paler than that covering the parts exposed to its influence; and it affords a striking proof of the truth of this theory, that some of the northern animals are dark colored in summer, and white or pale in winter.

The human being is equally dependant for its color on the influence of light.

The color of the skin depends on the color of the rete mucosum. This is white in the people that inhabit the north of Europe, copper colored in the American, and black in the Negro. The cuticle, which is of the same color, and equally pellucid in the different species of mankind, appears to contain no oxygen or light, and is most probably composed of carbon, hydrogen, and nitrogen. The rete mucosum is probably composed of carbon, oxygen, hydrogen, and nitrogen. The comparative quantities of carbon and oxygen in it appear to occasion the differences of its colors. These quantities depend on the quantity of oxygen attracted from it by light.

Light acting on the rete mucosum of the African, is continually subtracting oxygen, the principle to which its whiteness is owing. When the oxygen is subtracted, the carbon becomes the predominant principle, and hence that blackness peculiar to the negroes and the inhabitants of the torrid zone. In the Americans, the inhabitants of Asia, and the southern Euro-

peans, the color varies from dark copper colored to pale tawny. These people are less exposed to light than the Negroes, consequently their skin contains a larger proportion of oxygen. The inhabitants of the northern countries, who are still less exposed to light, are white: their rete mucosum contains its full proportion of oxygen.

In Europeans we find the parts of the body exposed to light, darker colored than those that are covered.

Women, who are less exposed to light, are fairer than men.

A subtraction of oxygen from the rete mucosum by any means, uniformly blackens it. The application of sulphure of potash blackens the skin almost instantly, as I have found by experiment. By combining with oxygen the rete mucosum is uniformly whitened. Dr. Beddoes whitened the fingers of a Negro by muriatic phosacid, which appears capable of giving out a small portion of oxygen and of still retaining all the light entering into its composition.

The different colors of different parts of the organic matter of the body depend chiefly on the light and oxygen entering into their composition.

The red muscles, as we attempted to prove in the last essay, are phosoxydated compounds, and probably owe their color to the light entering into their composition. From the white color of the nerves, and their office of conducting, and being excited into sensible action by condensed light, we may conclude that they contain no light in their composition. The arterial blood owes its fine color to the light and oxygen entering into its composition ; the venous blood is black from a deficiency of light and oxygen and a superabundance of carbon.

The colors of the cheeks depend on the number of blood vessels in the cutis, the quantity of blood circulating through them, the color and thickness of the rete mucosum and the thickness of the epidermis. Hence those people who

have a thin epidermis, a white rete mucosum, and a number of blood vessels in the cutis, have very rosy cheeks. When the veins on the surface of the cutis are more numerous than the arteries, the color of the cheeks will approach more to dark red or purple; when, on the contrary, the arteries are more in number than the veins, the color will approach nearer to fine vermilion, which is the hue of health.

Whatever increases the general action of the arterial system, reddens the color of the cheeks: general stimuli produce this effect. Certain passions of the mind more peculiarly occasion an increased action of the vessels of the skin of the cheeks.

Though the differences of the color of the skin in different nations must have originally depended entirely on the chemical influence of light; yet when these colors are once produced, their changes are in some measure dependant on the mind. When certain colors are considered as beautiful, the generating

imagination makes them hereditary, and the chemical changes from the influence of light are more slowly produced. Thus Europeans, though exposed to light in the African countries, do not become black, but in a great length of time ; and Negroes, though deprived of light, their accustomed oxygen attractor, are not blanched for many generations.

ADDENDA.

The experiments on the generation of heat were made long before the publication of Count Rumford's ingenious paper on the heat produced by friction. His experiments alone go far to prove the non-existence of caloric, and when compared with the second and third experiments in my essay, will, I should conceive, leave no doubts on the mind of the impartial and philosophic reasoner.

As vegetables depend for their life on a certain equilibrium between their principles, they must be continually supplied with the different substances by which this equilibrium is maintained.

A supply of water, carbonic acid, light, &c. is essential to the true vegetative process. The greater number of vegetables die in a short time, when deprived either of water or carbonic acid.

I should suppose, that if it were possible to deprive carbonic acid, hydrogen, and nitrogen, of the water they generally hold in solution, they would be soon destructive to all vegetables. The gases that I made use of in my experiments probably contained much water, as I always filled my vessels under water, and then transferred them to a mercurial trough, the surface of which was covered with water saturated with the gas with which the experiment was to be made. After the insertion of the plant, the height of the mercury was marked; from whence the absorption of gas could easily be known.

In carbonic acid confined in this manner under mercury, many vegetables live for a considerable time. Among these vegetables are

the different species of *arenaria*, the *chironea* *centaurium*, and the *sedum anglicum*.

As I was obliged to be methodical in my last essay, I have only mentioned one experiment to prove the decomposition of carbonic acid by plants, that on the *arenaria tenuifolia*, because it was the most conclusive. I do not, however, suppose that the whole of the phosphygen produced in that experiment arose from the decomposition of carbonic acid ; some of it probably arose from the decomposition of the water dissolved by it.

Some plants die in carbonic acid in ten or twelve hours ; among these are water plants in general, and the different species of *rumex*.



The generation and growth of vegetables, though apparently very complex processes, depend on the simple laws of attraction and repulsion.

The apparent regular expansion of a vegetable germ is produced by its power of attracting and assimilating to itself a number of principles. Vegetation only differs from crystalization, as being a more complex process. In crystalization certain particles, homogeneous in relation to each other, combine together according to the laws of their reciprocal attractions and repulsive motions, to form regular figures. In vegetation, a compound substance (that is, a germ) is capable of attracting different principles. When these principles are assimilated to it, it possesses the power of attracting new ones, and thus shoots out into leaves of different forms, and continually gaining new attractive powers, at length produces flowers and fruits.

To illustrate this, we will suppose a germ of a certain figure, composed of carbon, hydrogen, and oxygen, in certain proportions. The combined attraction of hydrogen and carbon for oxygen, and that of oxygen for hydrogen and carbon, enables the germ to combine with water and carbonic acid. These principles become assimilated

lated with it, it consequently encreases in bulk, and rises above the foil which gave it birth. When acted on by light, a new chemical process takes place; the superabundant water and carbonic acid, the oxygen of which rendered it white, are decomposed by the attraction of light for oxygen, and probably by the mutual attractions of hydrogen and carbon for each other. A new equilibrium of principles now takes place; the plant becomes capable of combining with portions of phosphygen, and its color gradually changes from white to green. In this manner the vegetative process proceeds, from the assimilation of new principles new attractive powers are continually gained, and different parts of the plant become of different forms and colors.

It is probable that the process of animalization is similar to that of vegetation, and different only as being more complex. Indeed, as the principles of living beings become more numerous, the delicacy of organization increases.

The combination of particles, homogeneous in relation to each other, produces regularity of form. The combination of different principles, according to certain laws of attraction, produces irritability. Sensibility may be the result of an infinitely more complex organization:

Almost all the vivid colors with which we are acquainted, belong to the substances which contain light. We do not, however, mean to assert that color is a necessary effect of combined light; on the contrary, many bodies that contain a considerable quantity of light in their composition, are white, black, or pellucid; white, as the phosphates, black, as the phosphoxyd of manganese, and pellucid, as the phosphacids.

To call any principle the coloring principle, is absurd. The colors of bodies, as we attempted

to prove in the doctrine of light, depend on their capacities for subtracting the repulsive motion of light. The colors of compound substances are found in general to be totally different from, and to bear no relation to the colors of their constituents. Two transparent substances combining, often produce an opaque body ; and two white constituents, a vivid colored compound.

ESSAYS
ON
HEAT, LIGHT, and the Combinations of
LIGHT,
WITH A NEW THEORY OF
RESPIRATION.
ON
The Generation of OXYGEN GAS,
AND THE CAUSES
OF THE COLORS OF ORGANIC BEINGS.

By HUMPHRY DAVY.

“ La lumière, ses combinaisons sont encore moins connues
que celles du calorique.

“ L'organisation, le sentiment, le mouvement spontané, la
vie n'existent qu'à la surface de la terre, et dans les lieux
exposés à la lumière.”

Lavoisier, Traité élémentaire.

SPECIMEN
OF
An ARRANGEMENT of BODIES
ACCORDING
To their PRINCIPLES.
By the EDITOR.

N. B. Philoxygena are ss. reliable.
Misoxygena are ss. unreliable.

I. LIGHT.

Electric fluid.
Galvanic fluid.

II. OXYGEN.

ox. with light ;
i. e. phosphygen.

III. 1. HYDROGEN.

Hyd. with oxygen ;
i. e. water.

2. AZOTE.

Az. with phosphygen
i. e. atmospb. air,
or gaseous oxyd,
or nitrous air,
or nitrous acid,
or nitric acid.

Az. with hydrogen ;
i. e. ammonia.

3. CARBON.

(qu. Azote with
hydrogen ?)

C. with oxyg.
i. e. carbonic acid.

C. with hydrogen ;
i. e. carbonated hydrogen,
or alcohol,
or æther,
or oils.

4. SULPHUR.

(qu. composition ?)

S. with oxygen ;

Metallic ss. that is, compounds of hydrogen and azote.

Alkalis.

Does the mode of union of their elements render them non-oxidable? or have they already oxygen or phosphygen closely combined?

REMARKS
ON THE
PRECEDING TABLE.

ITS ORIGIN.

This scheme was first printed in spring 1798, in illustration of part of a course of chemical lectures, which the desire of philosophical information, manifested by many persons at Bristol, induced the editor to undertake.

It was my design to present a number of examples, just sufficient to render my ideas intelligible ; by no means to digest all chemical facts into a table. The only alterations made for the present impression, are those which the discoveries related in the foregoing papers rendered necessary.

WHY CALORIC IS OMITTED.

Long before I was acquainted with these discoveries, or Count Rumford's experiments on friction, I had expunged the *matter of heat* or *caloric* from my chemical system. And on this occasion it is but justice to attest, that the author of the former derived no assistance whatever from the Count's ingenious labours. My first knowledge of him arose from a letter written in April 1798, containing an account of his researches on heat and light: and his first knowledge of Count Rumford's paper was conveyed by my answer. The two essays contain proofs enough of an original mind to make it credible that the simple and decisive experiments on heat were independently conceived. Nor is it necessary in excuse or in praise of his system to add, that at the time it was formed the author was under twenty years of age, pupil to a surgeon-apothecary in the most remote town of Cornwall, with little access to philosophical books, and none at all to philosophical men.

What induced me to reject the almost universal opinion concerning caloric, was principally the strange abuse of the doctrine of *latent heat* by Mr. Lavoisier. This I consider as a compleat *reductio ad absurdum* of the hypothesis of caloric, and a humiliating example of the frequent inability observable in men of the most energetic understanding, to push their scepticism far enough. Experiments on the capacity of bodies for heat, demonstrate that there cannot be condensed in nitre enough of caloric for that generation of gases and increase of temperature which are observable in the firing of gunpowder and of other explosive compositions. If there occur propositions in the same system not quite so glaringly contradictory, still it cannot be maintained without many assumptions perfectly gratuitous. And, unless we suppose a change of capacity, or the condensation of oxygen, the phænomena attendant upon friction and percussion appear totally inexplicable. To posterity those pages in the history of science will not appear the least extraordinary, which exhibit almost all the philosophers of our inno-

vating age as more than acquiescing in an old theory or arrangement of facts which excludes the most capital of all.

Most of the phænomena relative to heat may be classed under five heads.

1. The change of bulk, for the most part correspondent to the variations of a particular sensation; and the difference observable in different bodies in this respect.

2. The manner in which adjacent bodies, unequally heated, affect each other's temperature, or the conducting power for heat.

3. The effect of the mixture of bodies unequally heated; or the doctrine of capacity.

4. The change of state in bodies in consequence of variation of temperature.

5. The excitation of heat; and particularly the relation of heat and light.

All these phænomena I found much more easily reconcilable to the *mechanical* than the *chemical* doctrine of heat.

LIGHT.

The part of my table belonging to light, is new. Before I was acquainted with the two preceding papers, I had no satisfactory ideas on the chemical affinities of this great agent. What I have now received into the arrangement depends upon the evidence contained in those papers. I had before considered light and the electric fluid as identical; and it is probable that the galvanic influence depends upon light attached to animal substances, as the electric fluid is found attached to some inanimate substances; and that it is excitable in a manner somewhat different. That the galvanic and electric fluids differ in their laws of motion, has been rendered probable by the researches of Dr. Fowler, of Dr. J. F. Ackerman, (*Versuch über die Lebenskräfte organisirter Körper Frankfurt. 1797*), Mr. Humboldt (*Versuche über*

die gereizte muskel und Nerven-faser. Posen. 1797), and others.

OXYGEN.

Oxygen, as well as the modifications of light must, I believe, be classed apart. So little analogy does their chemical action bear to that of any other substance.

CLASS III. AND IV.

I could think of no principle by help of which so conveniently to arrange other substances as their relation to oxygen and phosphygen. I doubt whether any arrangement will ever be devised, equally suited to the most numerous and essential chemical properties of bodies. Whatever arrangement be observed, it will happen here as in the botanical indexes of Linnæus and other naturalists; like bodies will sometimes be separated, and the unlike be brought near. The greatest blemish in the present table seems to me the disjunction of ammonia

from potash and soda ; but we have the advantage of classing potash and soda with the other fixed alkalies (namely barytes, strontites, and lime), from which they have heretofore been very unphilosophically parted. Dr. Cullen's distribution of the objects of chemistry into salts, earths, inflammables, metals, and waters, has obtained mostly in this country in consequence of its adoption by Dr. Black. It was well suited to the state of knowledge for the time being. The classification of Fourcroy and the modern foreign writers of compends of chemistry exhibits, in my opinion, small ingenuity or comprehension. The phænomena in which light and oxygen are concerned, especially as the actions of life promise to be comprehensible under them, will probably long constitute the most curious and important part of chemistry. And if they afford a convenient and extensive principle of arrangement, it ought unquestionably to be followed. Those who compare it more studiously than I have done with the whole mass of facts, will apply it more happily in the subdivisions.

More than mere classification, I had it in view to place under the reader's eye certain probabilities that might lead to the analysis of different bodies, at present considered as simple.

CARBON.

Mr. W. Henry's late experiments seem to have invalidated the conclusions of Dr. Austin. Nevertheless, I have placed a query after *carbon*. No positive effect adequate to the immense provision of azote can, I believe, be pointed out, if it be not absorbed by vegetables to form charcoal. It may be questioned whether the decomposition of carbonic acid be equal to the supply of that constituent part. This however is a mere surmise.

Experiments on vegetation in which the atmosphere should be excluded, and the water should be confined by quicksilver, would throw light on this question, because then the access of charcoal and carbonic acid might be pre-

vented, and only known substances admitted. There is another reason for suspecting charcoal to be a compound. If phytivorous animals do not take in azote by respiration, they may obtain it from the charcoal of the plants in which they feed. From the plants they must obtain it.

If we could fully rely upon the experiments, from which it has been inferred that organized beings, both animal and vegetable, can subsist upon air and water, (and if atmospheric air, consisting of phosxygen and azote, is to be understood), it would be probable that sulphur and phosphorus consist of hydrogen and azote. For animal substances contain phosphorus, and they yield sulphur during putrefaction. I am not sure, however, that this latter fact has been ascertained with respect to the substance of animals confined to air and water. In such a case, however, I suppose we may argue from analogy:

SULPHUR AND PHOSPHORUS.

The fusibility of sulphur and of phosphorus appears to me to afford some hope of their decomposition. It certainly renders them much more manageable by the chemist than charcoal. A project for this purpose some time ago occurred to me; and if no one publishes an account of the failure or success of experiments such as I have in view, something on this head may appear in the next volume of the present collection.

MURIATIC RADICAL.

The summary which Dr. Girtanner has published of his experiments (*Antiphlogistische Chemie*; Berlin. 1795. s. 154), goes a very little way towards proving that hydrogen is the base of the muriatic acid. But this acid is copiously formed in nitre-beds; and (what is more in point), the product of experiments in which Dr. Priestley fired hydrogen and oxygen with some azote in close vessels, was found by Mr.

Keir to be muriatic with nitrous acid and water. Hence I suspect the muriatic to be a compound radical. Certainly if we can rely upon the purity of the substances employed by Dr. Priestley, it must be hydrogen or azote, or compounded of both. That it is not hydrogen alone may in some measure be presumed from the result of those laborious experiments in which hydrogen and oxygen gas, as free from azote as they could be procured, were burnt together without any apparent production of muriatic acid.

FLUORIC AND BORACIC RADICALS.

Having nothing particular to observe of these, I have not placed them in the table.

METALS.

In conformity with the above related experiments on light and oxygen, and also with the principle of this arrangement, I have thrown the metals into three subdivisions. The first

group contains such as seem to combine with phosphygen only : viz. platina and gold. Silver, lead, and manganese, evidently combine with oxygen and phosphygen ; the first being soluble in sulfuric acid, and the two others yielding phosphygen (oxygen gas) on application of heat, without reduction. The nature of the rest is uncertain. Every fundamental discovery necessitates a revision of facts ; and it is to be hoped that Mr. Davy himself will find leisure for a train of experiments on oxydation and phosphydation.

Concerning the composition of metallic substances, not much can be said. Whether to create a diversified system of bodies out of one, or out of a few or many elements, imply most wisdom and power, is a question which different persons would decide according to their various taste in world-making. And the dispute might go on without hope of termination for as many centuries as have elapsed since Thales the Milesian to the present hour. But we have some indications that the metals are not so

many simple substances ; and in the case of some among them, by accurate *close* experiments on organised bodies, we might have certain proofs.

The existence of iron in such variety of plants and animals ; and of manganese in some plants, suggests an opinion that these metals are compounded by the organic powers ; and then we are warranted by analogy in surmising that the other metals consist but of the same principles differently modified.

CLASS IV.

If future experimenters should accomplish the oxydation of any of the bodies of the fourth class, such bodies must be transferred to the third class. Should it be discovered that oxygen enters into their composition, the terms *phil-oxygenous* and *mis-oxygenous* must be changed ; and it is probable that in the present state of facts, more apt titles might be found for the two last classes.

POTASH AND SODA.

The existence of several of the bodies themselves of the third and fourth classes, or of their elements, in so many organised beings, has often suggested the idea of the formation of the bodies by the chemical processes of life. Thus a late author: “ These experiments, I
“ think, shew that vegetables and animals
“ possess organs, capable of assimilating to their
“ own nature, the matter destined to their
“ nutrition ; that the animal powers can prepare salt, lime, and iron, which are found to
“ exist formally in the body ; however the
“ greater part of an animal or vegetable is without such substances, yet, when destroyed by
“ fire, its component parts do in general recombine, and thus produce those substances.
“ Since, then, animal matter is only a peculiar
“ arrangement of common matter, why may
“ not the organs of imperfect animals be capable, as well as vegetables, of producing this
“ arrangement? It seems probable that they

“ are capable ; but animals of more perfect
 “ organization who possess sensation, and were
 “ designed by nature to live on previously pre-
 “ pared animal and vegetable matter ; they
 “ appear less able to accomplish such conversion,
 “ and die ere it is effected, from the derange-
 “ ment of their sensitive organs.” (Abernethy’s
 Essays. 1793. p. 102, 103.) Again : “ I had
 “ imbibed the opinions of the great philosophers
 “ of this island, who from reflection and rea-
 “ soning were induced to believe that the ulti-
 “ mate particles of matter were the same ; and
 “ that the various substances with which this
 “ world presents us, were only differences in
 “ the arrangement and motion of similar par-
 “ ticles. The testimony of experiment appears
 “ to me to be now added to the truth of an
 “ opinion, formerly supported merely by the
 “ suggestions of reason.” (Ibid p. 104.)

The opinion has sufficient plausibility to
 deserve to be properly ascertained ; but it has
 still I fear no support but in conjecture, which
 is probably all to which “ suggestions of rea-

son" amount. If however the gold fish which Dr. Fordyce kept for six months in distilled water were placed in the same situation as the subjects of Mr. Abernethy's observations, it can by no means be concluded that they subsisted on atmospheric air and water. The atmosphere, being a vast magazine of dust, vapours, and gases, may yield to plants almost whatever they can draw from the soil.

Whether some of the matters of the fourth class exist in organized bodies, or are formed by processes in which no material is added, is doubtful. Nor is it of any consequence to the hypothesis. Those matters will in the latter case equally consist of the elements that enter into organic bodies.

Some recent naturalists have adopted the doctrine of equivocal generation in consequence of their own observations. Among others, Leske, in his travels through Saxony (*Reise durch Sachsen*). If there arise, in the vicissitude of things, equivocal masses capable of arranging

the first principles of bodies into organic compounds, these, if not necessary to the existence of vegetables, may be expedient to their flourishing state. The effect of some manures seems to depend on the preparation of nutriment. There are facts which shew that the organic powers, rather than form certain constituent parts, will receive them when presented ready formed. Sometimes they receive them in a compound state, and afterwards decompose them, rather than compound them anew, though capable of doing so. This is true of that product which in whatever state it exists in vegetables, appears as alkali after incineration. Thus Jacquin, in his elements of chemistry, informs us that the *salz kali*, which in maritime situations and in salt soils yields soda (probably from the decomposition of muriate of soda), when growing so far inland as the vicinity of Vienna, yields potash.

LIME AND SILEX.

On the probability of the organic formation

of lime, alumine, and filex, it is needless to expatiate. The facts which favour such an opinion cannot be unknown to any student of chemistry.

In a paper in the *Journal de physique*, Mr. Dolomieu has endeavoured to shew by experiment that hydrogen enters into the composition of filex. Several years ago, however, I kept small pieces of filex boiling for a considerable time in heated oxygen gas. No combustion was observed, and the several pieces weighed as much as before they were heated. They had only acquired a spongy texture. Evidently as they appeared to bubble while hot, and to be porous when cooled, not a particle seemed to evaporate at this high temperature. The opinion of the composition of flint therefore, at present seems to have no foundation but in the well-known facts relative to the bamboo.

Possible use of the present Conspectus.

Should the present view lead, in but a single

instance, to successful experiments on the decomposition of bodies of unascertained constitution, it would be a great advance towards the removal of the present difficulties in chemical theory and practice. These difficulties are chiefly of two kinds. On the one hand we are limited in our power to change the forms of substances by the firm, and hitherto impenetrable structure of metals, earths, and of some other bodies. Their decomposition would doubtless be followed by the creation of numberless new arts, and a great change in the condition of man.

The other species of difficulty is much more worthy of the efforts of the enterprising and the ingenious. To overcome it would contribute infinitely more to the advantage of human society. This difficulty is formed by the complicated and ever-changing texture of living matter. Sound philosophy leads us to suppose that here, as in inanimate matter, action depends upon composition ; and variety of action, of course, upon variety of composition. In the

constitution and qualities of nerves and muscles, there may possibly, within the limits of the living state, exist as wide a difference (I do not say the same difference) as between the *nitric* and *nitrous* acids. When we consider the attention at present paid to the phænomena of organic nature, we may be sure that it cannot be long before the reality of such variation is demonstrated by specific experiments. It must be known in all its range before the practitioner of physic can be considered as an artist, guided by fixed principles. And this is but the first requisite. For the nature of the deviations from the healthy constitution must be ascertained, before any one can go about to correct the diversified unhealthy constitutions with a sure hand. Till advances therefore are made in *chemical physiology*, *medical science* must continue a chimera.

CASES

Of GONORRHŒA treated with

MURIATE OF QUICKSILVER.

CASES of GONORRHŒA

TREATED WITH

MURIATE OF QUICKSILVER.

WEST BROMWICH. 1798.

Dear Sir,

I am sorry that the results of the several trials which I have made according to your recommendation, of the power of nitric acid in the cure of syphilis, have not been sufficiently precise to occupy a place in your intended report on this subject. Most of the patients to whom I have prescribed it have made too much difficulty, or have been too irregular in taking the medicine, to afford grounds for any satisfactory conclusion, either in favor of its efficacy, or against it. At this time, indeed, a venereal patient is employing it with great

regularity, and as far as the evidence of a single case still under care can be admitted, that evidence is strongly in its behalf. It is a case of confirmed lues, of near five months standing, which has resisted the operation of mercury, and, together with the remedy perhaps, has extremely reduced the general health of the patient. Both this and the specific disease are very much altered for the better in the course of little more than a fortnight's use of the acid, and there is now every reason to hope that the case will speedily terminate much to the satisfaction of the patient, and to the credit of the medicine.

On another subject which I mentioned to you when I had lately the pleasure of seeing you at West-bromwich, and on which you did me the honor to request a particular detail of the observations I had made, I can now furnish you with a more precise and satisfactory statement. I mean the use of muriate of quicksilver in the cure of gonorrhœa. By the following cases copied from memoranda made of them

respectively as they occurred, you will perceive that in the account I gave you I did not over-rate the success which has attended this curious, and in the hands of medical practitioners at least, I believe, novel practice. It was introduced to my knowledge by the subject of the first case annexed, and you will not be surprised, that on the first proposal of the remedy I shrunk with fear from the administration of it. Most men, perhaps, in the profession, would have done the same. But the urgent desire of the patient himself at length prevailed, if not over my timidity, at least over my reluctance ; and indeed I found that whether I was willing to prepare it for him or not, he was resolved upon taking it ; and it must be confessed, upon no insufficient grounds, if the account of the person who recommended it to him could be at all depended upon : the recommendation was accompanied with an assurance that "*it had been administered to hundreds, and always with success.*" This person, it seems, had been a soldier in America during the last war, and had there both learned the use, and witnessed

extensively the good effect, of the medicine. With its previous history I am no farther acquainted. What I have since ascertained will best appear in the succeeding list of cases. In these, sir, you will see that I have occasionally varied the medicine a little, both in its dose and mode of composition. To this I was led, sometimes by the fear of irritating too powerfully a constitution supposed to be more feeble and delicate than others, and sometimes by a wish to ascertain how far the exact formula as given to me in the first prescription, was essential to its success. The latter appears in the results; as to the former, I have to say that I have not found in a single instance, the smallest inconvenience arising from the exhibition, nor even a painful sensation, extending beyond a few hours at most of its active operation. In one or two, but I cannot now say which of the patients, it produced vomiting; in others only some ineffectual efforts to vomit. I have stated impartially, I believe correctly, and except in the first, as concisely as I am able, *all* the instances in which I have employed the

medicine in gonorrhœa.* The conclusions arising from the cases No. IX, and XII, are vague and indeterminate, though it is most probable that the medicine succeeded in both of them. Nos. III and VI, are perhaps at variance with the rest. The other nine, I think, afford an evidence no less satisfactory than striking.

The whole, sir, are much at your service to be made such use of as you shall think proper. Your zealous and valuable labours in the promotion and diffusion of medical science, entitle you at once to the thanks of the whole profession, and of mankind, and to the unreserved communication of every important fact which

* A patient of mine with old and obstinate strictures in the urethra, and an extremely irritable state of that canal so as often to occasion a long and painful spasm of the sphincter vesicæ and consequent suppression of the urine, was desirous of trying its effects in his case.—It produced the salivation as usual, but left the disease unaltered.

may at all coincide with the objects of your more immediate attention.

I am, Sir,

Your obliged friend and servant,

J. ADDINGTON.

CASE I.

JOSEPH M——, aged 35, a healthy mechanic, came under my care on the 14th of August, with a gonorrhœa which soon after its commencement, was marked by the usual symptoms of inflammation of the urethra, and swelling of the lips of its orifice, copious discharges, heat of urine, chordee, painful erections of the penis, &c. &c. in that degree, which commonly obtains for the disease the epithet of virulent. The usual mild, mucilaginous and laxative medicines were prescribed and regularly taken for

some time, and in the mean while the man being very desirous of a speedy cure, was extremely careful to avoid any irregularities in diet or modes of living which might impede his recovery. No material advantage however was obtained by all the care that could be taken. Mild injections, as of tepid milk and water, and very weak solutions of lead, and frequent immersions of the penis in these liquids were employed in their turn to no better purpose. Every means that could be devised was adopted for rendering the urine mild by larger dilution, and as I suspected that the patient's sufferings on this score might be augmented by a peculiar disposition to the formation of acid in his stomach, affecting him with almost continual heartburn, he drank daily a considerable quantity of aerated alkaline water, which assisted somewhat in the mitigation of them.—But in spite of every effort, the disease continued in a state of painful activity, and after a few weeks, the patient's stomach was disposed to reject every medicine. Things were in this state at the end of two months, when the man requested

my permission to take a medicine which an old acquaintance had recommended to him, and which was said to have cured *hundreds of patients* in this disease. He was promised that it should cure him by taking only *two or three doses in the course of a week or ten days*. Its mode of operation was described to him in the following manner:—that upon taking it at bed-time, it would in the course of a few minutes excite a great burning in his throat and stomach, and presently produce a very copious spitting to the quantity as was asserted “*of half a chamber-pot full*”—Upon looking at the recipe which he brought me in order that I might prepare it, I was startled, and expressed my apprehension that it could not be administered with safety; but the eagerness of the man to obtain a cure, and his confidence in the report of his friend, prevailed over both his fears and mine, and I made up the medicine according to the following formula:

Take of

Corrosive sublimate of mercury, 3 grains,
Rectified spirit of wine, 1 ounce.—Mix.

Half of this solution was to be taken at bed-time. On the morning of the second following day he was ordered to take an ounce of Glauber's salts, and in a day or two more to repeat the draughts, and afterwards the salts as before ; these, it was expected, would be sufficient to complete the cure ; if otherwise, the medicine was to be repeated at the same intervals, with a dose of the salts betwixt every draught, till it was effected.

He took the first dose, viz. one grain and half of the muriate in half an ounce of rectified spirit, undiluted, Oct. 14, at going to bed. Its immediate operation on the fauces and stomach was exactly what he had been led to expect—a copious salivation was quickly raised, and lasted from an hour and half to two hours, during which time he said he spat more than a quart. The remainder of the night he passed as well or better than usual, and neither then nor afterwards felt any sort of inconvenience from the medicine. On the next day his complaint was very much relieved. The pain, soreness, heat

of urine, and discharge, were all lessened considerably. In the morning following, viz. the 16th, he took the salts dissolved in gruel, and in the evening of the 17th repeated the draughts with the same effect as before, and with equal proportionate benefit. The disease, though not altogether subdued by these two doses, was yet so much lessened as to give little or no trouble. However, wishing to be thoroughly rid of it as soon as possible, the man was desirous of taking on the medicine till it should be perfectly eradicated; and accordingly took four draughts more, viz. on the 21st, 26th, 29th, and once afterwards, but on what day I do not know. To me it appeared that the two last doses, if not three, were altogether unnecessary, the disease being then reduced to a mere gleet, which from the severity and long continuance of the inflammation might be expected to remain after every active symptom had disappeared: from this time, however, the man remained perfectly well.

CASE II.

ROBERT S ——— aged about 40. Gonorrhœa with the usual symptoms, of some weeks continuance, but not so violent as in the former case.

Feb. 28th, 1796. He had two of the draughts and salts as prescribed in case No. I, and was directed to take one that evening at bed time, and the other three or four nights afterwards, with the salts in the interval. He came again on the 7th of March. The medicine had operated as in the foregoing case, and the disease appeared so nearly subdued as not to need a repetition of it. The man, however, was very desirous of two more draughts, which were given to him, with the salts as before. This patient did not come to me any more, and soon after left the country. But I considered his cure as effected in the first eight days; which was the more remarkable as he had been under my care for the same disease in the course

of the preceding year, and was cured in the common way, in the space of about *two months*.

CASE III.

M. C — aged twenty-two. This gentleman applied to me with what he called the remains of a clap, of which he had supposed himself cured in London, a week or two before, but which he found still existing in a slight degree. I had no opportunity of examining him, as he thought his own account of the complaint would be sufficient to direct my prescription. I ordered him the same draughts as in the other cases, only adding two drachms of water to each. He took the first on the night of April 28th 1796, which had no effect whatever either on the fauces, salivary glands, or on the disease. He repeated the dose on the 30th, and this made no more impression than the former. I had afterwards reason to believe that this was a case of strictures in the urethra ; but as the gentleman left my

neighbourhood (being only on a visit) immediately after it, I did not ascertain this myself with certainty by means of a bougie, but recommended him to do so. His gonorrhœa had been treated with injections in London, and I have very often observed strictures to follow the use of those remedies *in the active stages* of this disease.

CASE IV.

— SHAW, aged twenty-five. This man also I had cured of gonorrhœa in seven weeks, in the common way, a few months before. This second infection brought him to me again May 4th 1796. He had two draughts, each containing the same quantity, that is, one grain and half of the muriate only, dissolved in proof spirit. On the ninth and tenth he appeared perfectly well.

CASE V.

Mr. J—— aged twenty. This gentleman's was a first infection, and extremely virulent.

The inflammatory symptoms were so urgent as to produce more than once, for some hours together, an entire suppression of urine. I contented myself therefore with employing the usual means of lessening inflammation for the first week, from June 24th till July 1st, and then began with the muriate. But as his constitution was extremely irritable, I gave him at first only one grain at a dose, dissolved in proof spirit. The salivation was considerable, though not so great as in case No. I, and the relief of all the symptoms in like proportion. The same dose was repeated on the 4th, with similar advantage, though the disease was not yet subdued. Finding this the case, and that the patient suffered no inconvenience from the medicine, I added half a grain of the salt to a draught, which he took on the 7th. The effect of this was greater, both on the fauces and on the urethra; and by three more doses, viz. on the 11th, 15th, and 27th, the disease was obliterated, except a gleet, which quickly yielded to an injection.

CASE VI.

JOHN B——, aged thirty-three. March 3d, 1797. Gonorrhœa in the usual form. This patient took six of the draughts, each containing a grain and half, but with less effect than usual; and finding himself not perfectly cured, as he had been led to expect, left me, as I suppose, to apply to some other practitioner.

CASE VII.

JOHN H——, aged twenty. Gonorrhœa ten weeks, not violent. Appearing of a weak and irritable habit, I did not venture on the usual dose at first. Feb. 15th 1797, he took one grain of the muriate, dissolved in three drachms of spirit of wine, and six drams of water, with good effect, and no inconvenience. On the 22d, therefore, I gave him a grain and a half in the same menstruum; in a few days a gleet only remained, which was cured by an injection.

CASE VIII.

Mrs. R——, aged twenty four ; a soldier's wife : had received a gonorrhœa from her husband a week or two before she applied to me. It was now in a state of considerable activity. Feb. 19, 1797, she took one grain dissolved in proof spirit ; salts on the 21st, and the same draught again on the 22d. These were sufficient for the cure, which was complet a few days afterwards.

CASE IX.

Mrs. Z——. I never saw this patient, nor knew the progress of the disorder, or the effect of remedies, but by the report of an intermediate person. She took four of the draughts, with one grain in each, viz. Oct. 19th, 23d, and Nov. 7th, and 12th ; and afterwards used an injection. No farther applications were made to me on her account after the 25th, when I suppose she was well.

CASE X.

JOHN WALTERS, aged 35. A virulent gonorrhœa, with hernia humoralis. I was called to him on the first of January, 1798. I attended to the disease of the testis for the first five days, and when this was on the decline, gave him a grain and a half of the muriate, dissolved in a drachm and a half only of spirit of wine, and an ounce and a half of water. The salivation took place as usual, and the disease was much lessened by the first dose. The dose was repeated on the 8th, 10th, and 12th, with salts on the intervening days. Both the diseases were now entirely removed, except that enlargement of the testicle, or rather of the epididymis which always remains a considerable time after the activity of the disease is gone.

CASE XI.

Mr. D——, aged thirty-five. Slight gonorrhœa three weeks. He took a grain and a half

Jan. 5th, 1798, in the same menstruum as the last patient. Another dose on the 8th, put a period to the disease except a gleet, which as usual was removed by injection.

CASE XII.

JOHN G——, aged about twenty-five. Jan. 14, 1798. Gonorrhœa in the usual form, of a fortnight standing. He had two draughts, with a grain and a half in each. I did not see him afterwards.

CASE XIII.

DANIEL H——, aged twenty-four. Recent gonorrhœa, with two or three small pustular eruptions about the scrotum and one groin. Having already given the medicine, not only in different doses, but also in different circumstances of solution in the menstruum, with nearly the same effects, I gave to this patient three draughts, each containing a grain and a half of the muriate, but dissolved in dif-

ferent proportions of the spirit and water. That which he was to take first had only two drachms of rectified spirit, with six drachms of water ; the next to be taken three days afterwards, had equal parts of them ; and the last to be taken in three days more, the same as the first. He was particularly desired to notice and compare their respective operation on his throat, salivary glands, stomach, and urethra. The first draught he thought acted the most powerfully in all respects ; the second and third much alike. By the three, the whole of the disease was removed, except a very trifling colorless gleet, for which when he came last I gave him an injection.

NOTE by the EDITOR.

Concerning the siphylitic case described in the beginning of the foregoing letter I must observe that a cure was not effected by the acid.

Mr. Addington doubted at last, whether it had diminished the disease, though it had greatly benefited the constitution.—I have heard of the recent employment of large doses of calomel, alternated with drastic purgatives in gonorrhœa; and the use of corrosive sublimate, as it is obvious to suppose, dates from old times, whence it has by tradition descended to our own. When the gonorrhœa appeared first, or was first attended to, as a symptom from venereal infection, the same remedy would naturally be employed as in other forms of venereal disease. Girtanner says “ the internal use of corrosive
 “ sublimate appears to be very ancient. In the
 “ 16th century it was the common remedy of
 “ almost all quacks. Physicians were as yet
 “ too timid to employ a medicine with whose
 “ formidable operation they were acquainted.
 “ Wiseman is one of the first that speaks of the
 “ internal use of the sublimate. He uses a solu-
 “ tion in water. Blancard also gave sublimate
 “ internally. Hermann of Leyden recommends
 “ it in gonorrhœa and gave two grains, made
 “ into a pill with extract of liquorice, for a dose.

“He properly remarks at the same time that
“this is a remedy only fit for strong constitu-
“tions. Girtanner’s Abhandlung uber die
“Venerische Krankheit Gottingen 1788. I.
“p. 361.”

CASE

By Mr. KENTISH, Surgeon,

NEWCASTLE-UPON-TYNE.

A C A S E

OF

MORTIFICATION of the TOES and FOOT.

Mr. C. a Tradesman in moderate circumstances, aged 60, in the beginning of November 1796, complained of violent pain in the right leg and foot. He had accustomed himself to pare off a horny substance from his heel, which when last done he said he had *cut to the quick*. On examining, it was found that the cuticle was off and the cutis dead for the size of a sixpence ; and on questioning I found they had been using all the means recommended by an officious tribe of neighbours to remove this *fitfast* (eschar.) The part being perfectly dry, the most urgent symptom appeared to be the pain he suffered in his leg. With a view of

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relieving this symptom he was ordered opium in large doses as recommended by the celebrated *Mr. Pott*;—also to take the bark freely with the intention of keeping up the power of the system to prevent further gangrene. The wound itself was superficially dressed with *ceratum saturni*. These means were continued for about a month without any relief—the wound increased in size and in depth, and now discharged a thin, foetid ichor. The pain still continuing as violent as ever, a fermenting cataplasm made with infusion of malt, yeast, and oatmeal was applied with a view of correcting the foetor: This application and the bark and opium were continued for another month. He now began to tire of the bark, and, finding he was not getting better but rather worse, he took on that peevish irritability so unhappy for the individual, and so unpleasant for the medical attendant. One of the toes soon afterwards spontaneously took on the same diseased action as had been induced in the heel by cutting away the horny excrescence; a slight vesicle appeared upon the *top* (I note this because *Mr Pott* says

it begins on the inside in general) of the little toe, without any apparent inflammation. I was not in a hurry to open it, as I did not doubt of the state of the part below ; and as it was not inflamed, nor so painful as the open wound, it was left to die and dry of its own accord. The wound increased in size very irregularly. It remained stationary on the inside, and crept along the outside of the foot as if in order to join its sphacelated relative, the little toe. When the edges of the wound appeared inflamed, and the suppurative process likely to take place, no separation ensued, and the surface of the inflamed parts died and came away with the discharge, putting on the appearance of a phagedenic ulcer. He had now been attended for upwards of three months, and had taken great quantities of opium and bark in a variety of forms. The applications to the wound had also been much varied ; emollients, stimulants, and antiseptics, had each been tried without the least apparent benefit, so that I own I felt as tired and disappointed as the patient himself. The dread of expence, and the little benefit received, added

to the irritable state of mind of the patient, induced me to comply with their implied wishes of resigning him to the care of his *quack friends and their nostrums* ; so that about the middle of February he was left to himself, and my attendance ceased. Some months after I saw his wife, who with an exulting smile, told me that her husband was a great deal better, and that he would be soon well now, as they had gotten some salve which had never failed in *curing sore legs*. She repeated a number of wonderful cures, with all the embellishments of female elocution. To do just credit to the inventor's abilities, I congratulated her upon the good fortune of getting such an invaluable nostrum, and added, with truth, that it would give me great pleasure if it accomplished her wishes ; saying, if they had no objection, I would call in from time to time to observe the progress of the cure. This they readily consented to, and I determined to remark the effect of their specific. Accordingly I called, and perceived the disease had made considerable progress since I had seen it ; the ulcer had not only reached the

little toe, but had crept over the upper part of the foot, near the base of the toes. What induced them to think it better was, that some parts where the ulcer had travelled, had skinned (without any appearance of granulation), and the cicatrix shewed the loss of substance. I begged them to continue, and joined them in their hopes of success. The wonderful ointment they used, I found came from Portsmouth Common. A number of extraordinary attested cures of men who had been discharged from the Royal Naval Hospital, and afterwards cured by this ointment, was sufficient to impose upon the weak mind of an invalid, and the superstition, weakness, and ignorance of their neighbouring gossips. They gave some of this ointment, which I found, upon analysis, was something nearly similar to *ung. resin. flav.* with the addition of a small quantity of finely levigated *æruugo æris*.

I found the patient's health much in the same state as when I ceased my visits, if any thing, rather better than worse. As the warm

summer months approached, his pain became more tolerable ; he had not taken any medicines since I saw him, and his diet was not too high or stimulating. His appetite was good.

In about a month after I called again, and found the disease still proceeding. The metatarsal bones were beginning to be exposed, the ulcer to spread upon the sole, and the phalanges being quite dead and insensible, they requested me to take them off for them, as that was beyond their surgery. Accordingly at different times I took the phalanges off: the disease still continued to creep on, until the metatarsal bones, and some of the bones of the tarsus were much exposed. A hæmorrhage occurred every now and then, which was slight, and subsided of itself, (N. B. He always felt relief from these spontaneous hæmorrhages for a few days after they happened). He had now made a trial of the famous means which were to cure him so speedily, of six months, without finding any benefit. The cessation of pain appeared to be more from the warmth of the weather than

from any other cause; and for the healing appearance of the wound, I apprehend it was more from some law of the system, of which we are as yet too ignorant to benefit, than from any specific power of the wonder-working ointment. A few cool nights in the beginning of September brought on a return of violent pain, and he now seemed willing to lose his leg, as the only means of saving his life, or rendering him free from pain. The ulcer had spread to the *malleolus internus*, and the inflammation for two or three inches higher. Both legs were also œdematous to above the calf. In consultation with my friend and partner, Mr. Abbs, he advised the limb to be taken off above the knee, for fear the same disposition might remain in the stump below the knee, as the periosteum of the *tibia* was already affected. Accordingly on the tenth of September 1797, the operation was performed in the now usual manner: much of the integuments were saved, advanced to cover the stump, and put up with sticking plaster, with a view of healing by the first intention. Having remarked the tendency

of every inflammation of the foot to have ended in the death of the part, I wished to avoid exciting the least possible unnecessary action of the parts divided. With this view I used nothing but cold water to the face of the stump. (I now believe I was wrong in the means, as water of the temperature of the stump would have excited less than that of a lower temperature, which would favor a re-action.) The femoral artery, at the part where it was tied, was studded with ossified points, and appeared to have lost much of its contractility, as it did not retract after the operation, but projected forward like a piece of gum-elastic catheter. On examining the amputated limb, the artery was found to have the same ossified appearance. My patient was put to bed and kept quiet; he passed a tolerable night; had a considerable flow of urine after the operation, and the swelling of the remaining foot and ankle had entirely disappeared before the morning.

He continued quite easy, and on the eighth day after the operation, the stump was dressed.

It appeared well and was suppurating kindly, and was now dressed every other, and sometimes every day, according to the quantity of pus that oozed from it.

In about a fortnight all the ligatures came away except the one upon the femoral artery : at this time every thing looked well. About a week afterwards he had both pain and tension of the stump, and in a few days an abscess formed, which discharged itself upon pressure from the dependant external part of the stump, which was very much contracted in its dimensions, so as to flatter me with a speedy cure.

The discharge continued considerable for several days ; it then diminished, and I was in hopes it was done ; but on the contrary, in a few days the whole stump felt inflamed, attended with some symptomatic fever. With a view of getting as dependent an opening as possible, I applied a caustic to the external and inferior part. When the eschar was cut through a great quantity of very good pus was discharged.

This continued, and the whole surface of the stump was in such an inflamed state as made me dread it would put on the same sphacelating tendency that it had in the foot. Experience in other secretions in wounds had convinced me, that to suppress them, the patient ought not to be excited with a view to his being supported under the discharge. Besides, I had my experience of the bad effects that stimulants, such as opium and bark, aided by culinary means, and their auxiliaries, port wine, and porter, had had upon my patient during the time of the disease in the foot.

Feeling myself thus interested for a favorable termination of the case, it made me reflect upon what I imagined to be the cause of the original complaint. My patient was about sixty years of age, had eaten and drank heartily, or in other words, lived well, though not in the highest rank of life.

From the dissection of the amputated limb, we clearly saw the arteries were beginning to

ossify : this disposition I apprehend was owing to their having been too frequently excited by stimulant food, and liquor. I do not know any remedy which will dissolve ossification of the arteries. I wished my patient to have taken the nitrous acid, with a view of having disengaged the phosphoric acid from these bony concretions, as I imagined a nitrat of lime would be more soluble than a phosphat of lime, which I suppose these ossifications are. What would have been the result I am unable to say, as my patient was much against putting himself under any regular course of medicine.

Having had such strong evidence of great disease in the blood vessels, and possessed with an idea that it arose from a too great, and a too long continued excitement, I was forcibly struck with the notion that the best mode of relieving the system would be, by taking some blood from the vessels. I therefore ordered him to be bled; and it was not without some difficulty I could persuade them to let my assistant bleed him. His wife and his neigh-

bours remonstrated loudly, and said I had more need to give him some blood. "What!" said they, "bleed a poor man who has been confined above a year, and is quite reduced to a skeleton! oh for shame!" However I persevered, and in a few days he submitted. The blood was very fizy; eight ounces were taken, and on the following day the appearances were better. He had an easier night, and the stump was less inflamed.

I received great pleasure from finding the good effects of practice regulated by principle. I therefore determined to pursue the same means, at least so as to determine, without room for a doubt, on the subject. As the stump still remained, and the abscess discharged a good deal of pus, I ordered a dozen leeches to be applied to the inflamed part, and gave a smart purge on the following day. This checked the inflammation very considerably, and in about a week after they were both repeated. I now found so much difference in every appearance, that I was well convinced I

was in the right road, and therefore determined to persevere. As he had for such a length of time been accustomed to a drain from his system, I ordered him an issue in the other leg, and put him upon a bland diet. He was blooded to the extent of eight ounces every week for six weeks, and took a purge occasionally. During this treatment he gained flesh considerably; his stump gradually ceased to discharge, and healed with simple superficial dressings: and in February, 1798, he was perfectly cured, being a year after his first application, the half of which time he treated himself. Ever since he has remained perfectly well, and has enjoyed a much better state of health than he had previously done for years. It is now December 1798; near a year since his cure.

As the practice pursued in this case was various, and that which appeared to be of the most use was so contrary to what has been recommended by authors, even those of the first character, some observations must naturally

arise from the comparison. The disease of my patient was evidently that so accurately described by Mr. Pott in his "Observations on the mortification of the toes and feet," and which frequently extends its ravages above the ankle, and generally hurries off the patient in spite of every effort of the healing art. Mr. Pott wrote his observations with an express view of recommending the use of opium in addition to the bark, as may be seen from the cases he relates.* As it is no doubt a very painful disease, the exhibition of opium, as recommended by Mr. Pott, when it relieves the pain is certainly to be pursued; but there are certain cases where it does not produce the desired effect, as in the one I have related. In such cases, in my opi-

* If the patient have been accustomed to great excitement by fermented liquors, the use of opium as a different stimulant, with a view of lessening the former one, may be a highly useful means. But it is too frequently the case in the practice I have seen, that when opium and bark are given, all the other high range of stimuli are conjoined—such as wine, brandy, volatile alkali, and above all, the richest or strongest animal diet with spices, &c.

nion, it had better not be continued. The bark was esteemed an infallible remedy for every species of gangrene, and was for a long time relied upon and exhibited in all forms, both externally and internally; but as it frequently failed, as it must be expected when it was applied to patients placed in the most opposite extremes of excitement, this in time made its powers doubted, and other remedies sought for. Mr. White, of Manchester, gives some observations on gangrenes and mortifications, in the 11th vol. of the London Medical Journal, in which he recommends musk and volatile alkali, and relates several cases in which the good effects are evident. It would require a volume to enumerate all the authors who have recommended different means for the treatment of gangrene; and it would only be a waste of time to mention them and their specifics—I will therefore wave all contradictory opinions of my medical brethren, and hazard one of my own from the appearances that struck me during my attendance upon the case related. I have seen several

and treated some cases according to Mr. Pott's plan, and must confess that all I had seen had terminated fatally. The want of success never struck me to have arisen from an irrational mode of treatment, but I supposed it was the nature of the complaint, as both from books and conversation with medical men, I found they were not more successful than myself. In the beginning of this case I set off in the old routine, as will be seen in the first part. When we had proceeded for three months, the patient was fearful of expence, and I was hurt at my want of success. This made me bestow some thought upon the disease, and endeavour to find out a remedy: after the operation, the state of the arteries appeared to me to throw much light upon the nature of the complaint. Although I do not mean to say that in all such cases the arteries are ossified, yet as the cause of the disease in this instance could be fairly traced as produced by that effect, according to the rules of sound logic, no further causes ought to be admitted; and though in some cases ossification may not have taken place, yet the disposi-

tion to take on that process may of itself produce the effect on the extremities.*

It appears to me that the diurnal use of fermented liquors and good animal food for a number of years, had induced such increased action of the heart and arteries as might, for a long time, have appeared as increased strength. We know it is a law of the animal economy, that if any muscle is much used it enlarges, and, to a certain point, becomes stronger, yet if this very strengthening process be carried to too great a length, it becomes the cause of exhausting that very power which it before appeared to give.† Thus do I suppose it to have

* Old people are subject to ossifications of the heart and larger arteries; and who can doubt but by too great excitement we bring on the appearances of old age, when from number of years it ought to be at some distance.

† Hard-working people in general, although they appear and are for a certain time stronger, very soon wear out; i. e. they bring on premature old age. Perhaps it will be found to be premature old age whether the system be too much excited by the too great exertions of the body from the stimulus of want, or the too great excitement of the stomach by indulging in profusion.

been with the heart and arteries of my patient : the excess of strength in the vessels ended in their weakness. The necessary consequence of exciting the stomach to greater action is also an increased action of the heart and arteries ; and if we see the larger vessels increased in action, we must take it for granted that the whole series will sympathize ; if so, the surface vessels of the whole system must feel the effect. As it is with increased action, so it is with diminished action : when the stomach is torpid, so it is with the heart and arteries, and all the surfaces of the body to a certain degree.* If the stomach be rapidly excited by spirituous liquors, the head is most frequently the part which sympathizes, if less rapidly the liver appears affected ; but if by gentle, gradual, daily excitement,

* Every practitioner must have had various opportunities of seeing this in gouty attacks of the stomach : the coldness of the extremities, the weak and fluttering pulse, cold sweats, and sometimes the relaxation of the sphincter muscles, cause trains of symptoms which can only be obviated by the prompt interference of a bold practice, founded on the knowledge of the laws of the living system.

the feet sympathize in what is termed gout, and sometimes in the gangrenous state of the toes and feet which the above case has related.

There are various surfaces capable of this sympathy, which we may mark every day in gouty habits, but all I believe proceeding from the same cause. It will be asked, would I pretend to cure gout by bleeding? to this I shall answer, that if my patient were in the torpid state, I certainly should not; as I am well aware that in that state even stronger stimulants than what had brought him to that point might be necessary, to prevent him from finishing in the last state of torpor, viz. death. Yet I must here remark, that to relieve is not to cure. Having relieved him by stimulants, to cure him even of the gout I would both bleed and purge, and make him lessen his diet gradually, so that his system might have time to recover that power which he had before incautiously wasted.*

* I must not be understood to assert that all cases of the gout would be relieved by bleeding and purging—No! I

With such views, and by such means have I cured my patient. I have had some other cases where the circumstances have been different, but the causes the same, and, by treating them upon the same principle, the complaints have yielded in a much speedier manner than when I have treated them in the routine—according to the nomenclature of nosologists.

am too well aware of the proteus-like symptoms of that complaint, and that we must act in many instances according to existing circumstances. In all gouty cases in which bleeding and purging will be of *more use* than any other means.

OBSERVATIONS
ON
CARBUNCLE,

BY

MR. YONGE, SURGEON,

SHIFNAL, SHROPSHIRE.

TO DR. BEDDOES.

DEAR SIR,

I wish you may think the following case interesting enough to deserve admission amongst your medical contributions.—The report is condensed as much as seems proper, and the few annexed observations will help to explain, if not to recommend the treatment.

Considering how long the beneficial influence of cold in variolous fever has been known, I am at a loss to account for the slow production of those important consequences which this discovery has furnished. Dr. Currie's interesting remarks upon the agency of heat and cold, have introduced the subject to more extensive enquiry and experiment; they recalled our old and frequent conversations upon the effects of cold in various diseases; and the case of your little

pneumonic patient, Miss Browne, where cold ablution so happily verified your prediction of its efficacy, and the great stress you laid in these diseases upon cold air.

In Savery's letters from Egypt, is mentioned a curious instance of the advantage of cold exposure in arresting the symptoms of plague; but amongst many public medical letters and reports which I have examined in the Philadelphia papers, I do not find any practical inference from the repeated suppression of their fever by the approach of winter, and I know only of one physician (Dr. Maclean) who tried the affusion of cold water in the West-Indies.*

* A physician to the army told the editor that during the flight of the British troops from the banks of the Waal to Embden (during that very severe weather when the French conquered Holland) the fever patients, who were much exposed to the frost in consequence of the mode of conveyance, and the precipitate movements of the army, all recovered. It is to be hoped that the details of this event will be placed on record. See the annexed remarks.

Editor.

As a topical remedy in inflammations, cold was long almost exclusively employed to mitigate the pain arising from recent scalds and burns : it has of late been more generally introduced in the treatment of other local affections; but I have reason to believe its beneficial effect in carbuncle not generally known, though ten years ago Dr. Withering prescribed it for a lady whose case is hereafter noticed. I have since had another opportunity (besides that offered by the case particularly related), of confirming the success of this practice, under which the patient recovered in his seventieth year; neither of these carbuncles were so extensive as in the present instance, but the efficacy of the application was in all equally remarkable, and I think deserving of record.

I am, dear sir,

sincerely yours, &c.

To

DR. BEDDOES.

W. YONGE.

SHIFNAL, Dec. 20, 1798.

CASE OF CARBUNCLE.

The subject of the following case is forty years of age, and of robust make and constitution : long accustomed to great and continued action of body and mind, and to liberal diet ; he had experienced during many years, occasional indispositions from various inflammatory affections ; such as rheumatic, or irregular gouty pains, (for more than one judicious physician doubted about their nature), uneasy sensations about the stomach and liver, often accompanied with febrile symptoms. He was also subject to gutta rosea, and to very irregular action of the heart. In the summer of 1795, after many hours of exertion and anxiety, half immersed in water, and exposed to heat, he was attacked in the night with severe pain and inflammation of the great toe, which continued during two or three days, with all the signs of perfect gout.

Symptoms of general inflammation then supervened, and the disease then assumed the character of rheumatism, from which he recovered with much hazard and difficulty. From that period his pulse became, and has continued, perfectly regular ; but rather feeble, and seldom less than 86—90 per minute. His other habits remained nearly as before, and during the last two years the equinoctial periods have been more distinctly marked by gouty and other symptoms, denoting considerable excitement of the system.

Under circumstances not very dissimilar from those preceding his former illness, a pimple arose near the middle of his back, in appearance like a small dark-colored variolous-pustule, very painful, hot, and excessively sensible to the touch ; its base inflamed, of a deep scarlet redness, about an inch in diameter, and the surrounding integuments indurated to the distance of two inches. Pulse in the evening, 90, in other respects well.—Six grains of calomel at bedtime, and a poultice of bread and milk to the affected part.

August 5th. Patient slept little ; pain severe ; disease rather increased ; has had copious discharge of very bilious stools ; pulse 98 ; four grains of calomel, and one of opium in the evening. Poultice continued.

6th. Pain much increased ; hardness and inflammation but little extended ; no sleep ; has had two evacuations similar to those of yesterday ; pulse 106 in the evening, but not full nor hard ; compresses of fine linnen dipped in cold water applied over the part, and very frequently renewed. Opium and calomel two grains of each at bed time.

7th. Cold applications relieve the pain and keep the patient tolerably easy ; had some sleep ; inflammation extends ; the color deeper, and hardness increases ; no swelling of the integuments, nor any signs of suppuration ; pulse 108 in the morning ; urine natural ; has had one very large bilious stool. Two grains of opium at bed-time ; takes saline julep ; gruel, and toast and water freely.

8th. Patient slept little, partly owing to the cold applications, which are almost continually renewed, to keep him tolerably easy ; pulse 120 ; inflammation extends rapidly ; very dark colored ; about four inches in diameter, with small vesicles on its surface ; bled to 10 ounces. Six grains of calomel this morning have produced three very copious and very foetid discharges of bile. Opium, saline medicine, and cold water continued.

9th. Quiet sleep, and freedom from much pain ; inflammation extends, but better defined in its circumference ; blood taken yesterday inflamed ; heat and induration very great, and no signs of distinct tumor ; one drop of healthy pus from the pimple this evening ; two very bilious evacuations ; urine high colored ; pulse 110. Opium and cold applications continued.

10th. Patient sleeps quietly at intervals, night and day, as the applications and consequent ease enable him to do ; takes mild food well ; pulse 108 ; one very copious discharge

of bile; good pus continues, now and then a drop; inflamed part about six inches in diameter; sensibility of the surface diminished; color near the centre almost livid: heat and hardness excessive; opium two grains night and morning; cold water continued.

11th. A very restless night; general heat and thirst; pulse feeble, 124; inflammation extends; heat of the part feels intense, and the patient only kept easy by almost unremitting application of cold water; discharge increases, a drop of good pus appearing on almost every compress—bled to eight ounces—four grains of calomel have produced three bilious stools; feet and ankles swelled in the evening; takes mild food plentifully; opium and cold water continued.

12th. Slept well at intervals; heat and thirst abated; pulse 110; blood inflamed; local disease extends slowly, but some projection of the inflamed part above the surrounding integuments, and near the centre is a little

softened. One bilious and very foetid evacuation ; healthy pus discharged more freely. Opium and cold water continued.

13th. Patient sleeps better ; pain diminished, and good pus flows abundantly from the opening pustule ; pulse 104 ; swelling of the legs and feet increases ; inflammation does not extend, and integuments rise. Two grains of calomel has produced one bilious evacuation ; opium and cold water continued.

14th, 15th. Opening expands, and discharge of good pus very great ; tumor well defined ; heart shaped, with the apex upwards, and measuring ten inches in its transverse diameter ; projection nearly equal over the whole surface, but hardly one inch in any part ; of a dark mahogany color, and seems livid towards the center ; surrounded by a scarlet zone, frequently varying in extent and intensity. Heat continues very great, and pain is only suppressed by cold ; appetite tolerable, and sleep better ; but the patient is much emaciated, his strength im-

paired, and the oedematous swelling of the legs increases—opium at bed-time, and cold water continued.

16th. This morning, enlarged the natural opening in a perpendicular direction, to near four inches. Wound bled freely ; dressed superficially, and the whole tumor covered with pledgets of saturnine cerate—*cold water discontinued*. Patient in the evening complains of excessive pain—inflammation increasing rapidly, and heat intense—cold compresses re-applied, and in half an hour the patient is easy. Opium, two grains at bed-time ; peruvian bark and aromatics every four hours.

17th. 18th. 19th. Disease varying frequently in its aspect : the scarlet zone alternately expanding and contracting. Cold applications at no time more remarkable in affuaging pain and restraining inflammation—discharge of good pus very profuse—bowels regular, but stools continue bilious—pulse 108--116—patient sleeps well, but his strength continues to decline—his

appetite is impaired—the swelling of his legs increases, and he sweats profusely—continues opium, bark, aromatics and cold water.

20th. Tumor softened and feeling spongy in its whole extent—two lateral openings made, beginning near the upper part, about an inch on each side the central incision, and carried, diagonally outwards, to the most depending parts of the tumor and through the substance of the diseased cellular mass—dressings of the mildest kinds, the whole covered as on the 16th, and water discontinued.

21. Recurrence of pain again induced the application of wet compresses, which relieved the patient as usual—discharge of pus very profuse—tumor diminishes and colour changing to a dusky brown—continues opium, bark, &c. and cold water over the dressings.

22. 23. Patient sleeps much, appetite improves and oedematous swellings diminish—

wounds look healthy, and membranous floughs begin to separate—bowels regular, and stools less bilious—sweats continue—bark with the addition of vitriolic acid, and cold applications continued.

24. 25. Patient sleeps much—discharge less, and floughs separate—inflammation nearly gone, and integuments begin to adhere—cold water discontinued, and bandage applied—sweats continue—two small pimples near the inferior edge of the tumor, inflamed and sore, resembling the incipient carbuncle.

26. 27. Suppuration and union proceed favourably, and the patient improves rapidly, but fresh pustules appear.

28. 29. Pustules increase in size, with pain, heat, and inflammation, having all the characters of small carbuncles—patient feverish—pulse 110, bark and aromatics discontinued—opium and calomel of each one grain every twelve hours, and the bowels occasionally excited by small

doses of rhubarb or natron vitriolatum—cold compresses to the painful tumors.

30. 31. Pustules less painful and suppurate successively, but new ones appear below—wounds healthy and union proceeds favourably. Patient improves in appetite, flesh, and strength; but sweats continue.

Sept. 1st, 2d, 3d. Union of integuments rapid, but the minor carbuncles exceedingly troublesome, and increase in number. Cold compresses and saturnine applications, as they seem respectively necessary; the skin being frequently smeared with olive oil.

4th, 5th. Fresh pustules spreading downwards, and the former ones successively suppurating, with the discharge of small membranous sloughs—cold water seems to keep them easy, but its effect less remarkable than in the first instance—wounds incure favorably.

6th. One of the tumors has within a very

few hours increased to five inches in diameter—swelling and hardness of the integuments considerable, but less heat and inflammation than in the first carbuncle. Pain severe; pulse 120. Six grains of calomel produced three copious evacuations—cold compresses applied, and renewed as at first.

7th. Discharge of sanious matter from the tumor, attended with *excessive* pain during a minute; subsides fast; pulse 106. Small tumors continue to spread and suppurate, the whole assemblage creating great distress and inconvenience to the patient, who nevertheless improves in his flesh and strength; swelling of the feet gone; sweats continue. Opium and calomel one grain every night, with mosch and valerian in larger doses three times a day. Cold water continued.

8th, 9th. New carbuncle reduced to its original size; patient in other respects as yesterday.

It is unnecessary to continue the diurnal report of this case. The small tumors continued to spread downwards over the loins, hips, and nates, suppurating successively, and varying in size from half an inch, to an inch and half in diameter. Neither rose water nor saturnine lotions were more efficacious than common water, to correct the occasional pain and excess of inflammation, nor did any applications check their production, which ceased not much before the final incarnation of the wounds, about the end of October. Mosch, valerian, peruvian bark, aromatics, and opium, were alternately employed as circumstances directed. In the middle of October, the patient became rather suddenly conscious of great diminution of strength, observing at the same time an apparent want of bile in the alvine evacuations, which continued of a whitish or light ash color during a fortnight, but without any change in the color of his skin or urine. He then discontinued all other medicines, and after taking a few grains of calomel, the stools resumed

their natural color, his strength returned, and he at present enjoys better health than usual.

Medical writers have distinguished two species of carbuncle by the terms *simple* and *malignant*; the last signifying those tumors attendant upon the plague, and on similar fevers; to the first kind only the following remarks are applicable.

The description of this disease usually found in books, impresses an idea of its necessary connection with a putrid state of the system; and the aspect of the inflammation helps to confirm this prejudice. In the true *anthrax gangrenosa*, mortification occurs amongst the first symptoms, or succeeds so rapidly as to preclude all hope of relief. But this is a very rare disease, and we may safely affirm, that in a great majority of cases, sphacelation is preceded by intense inflammation, and generally is the consequence of it. In the present instance, the great change

of color that took place on the 7th, 8th August without any appearance of swelling, or other sign of suppuration, gave reason to fear that some destruction of the integuments must ensue from sphacelation; and I believe this would have been the inevitable consequence of any cordial antiputrescent plan adopted to prevent it. But though excess of inflammation constitutes probably the most frequent source of danger, another still greater may result from debility, and deficiency of living power, as sometimes happens in aged and infirm patients; the circumstances of each individual case must therefore determine where, and to what extent, evacuants ought to be employed. Of these, bleeding, the most obvious and certain, is not frequently used, owing chiefly to the prejudice before mentioned. On the cure of *anthrax simplex*, Sauvages observes, "*raro phlebotomia indicatur nisi a vigore pulsus in hac specie non nocet;*" but the late Mr. Bromfield, one of the best modern writers on this disease, decidedly recommends bleeding, even under a languid state of the pulse, if no other symptoms of debility correspond to forbid it.

I have lately been informed by Dr. R. Darwin, of the unexpected recovery of an old person from bad carbuncle, after dangerous hemorrhage from the nose; and now am inclined to believe that venesection at an earlier period of the present disease, might have been attended with advantage.

The excessive sensibility of the part of the deep seat of the disease, and the little chance of success from simple resolution, may perhaps be objected to topical bleeding. I have no experience of its efficacy, nor find it recommended; but have heard that free incision through the whole extent of the indurated parts, at a very early period, has changed the character, and stopped the progress of carbuncle.

The cellular or adipose membrane is the primary seat of this disease, which is quickly communicated to the skin. Linnæus defines it, "*glandula subcutanea inflammata in suppurationem malignam vergens.*" The acknowledged efficacy of mercury in *variola* and some other inflam-

mations, may alone recommend this evacuant in similar cases ; but experience of the beneficial influence of calomel in habitual indispositions of this patient, confirmed those motives of preference which looser analogies suggested ; and one circumstance particularly induced perseverance in the use of this remedy. Dr. Darwin remarks that gout and rheumatism are not primary diseases ; but arising from a transferred morbid action of some other part of the system, and generally of the liver ; no previous indication either of torpor or increased action of that viscus was observed ; but the patient had been subject to hepatic affections ; and the copious discharge of bile throughout the whole inflammatory period, seemed to indicate a connection, which the subsequent deficiency or change of secretion confirmed.

A lady about forty years of age, and very liable to inflammatory affections, was attacked with pain and soreness in the region of the liver, feverish symptoms, and discharge of bile from the stomach and bowels. She was re-

peatedly bled ; the spontaneous evacuations were encouraged, and such additional remedies employed as promised to relieve her ; after continuing ten days in this state, a carbuncle appeared between her shoulders ; the bilious symptoms then soon subsided, and with the application of cold water, she recovered after a few weeks confinement. May we infer from hence that anthrax, like gout, gutta rosea, urticaria, &c. is sometimes the offspring of hepatic disease ?

Little remains to be observed relative to the medical part of the report. A physician, whose candor and judgment confirmed us in the general plan, advised musk and valerian as substitutes for peruvian bark and aromatics, with a view to support the strength by a new stimulus, as the usual ones were withdrawn, and from their repeated use in mortification of the extremities. A remedy so powerful as opium, and whose effects are so well known, needs no recommendation ; and it is hardly necessary to

add, that the dietetic regimen corresponded with the variation of the medical treatment.

The topical remedies recommended in carbuncle, are such as might be expected from the gradations of its appearance and character ; varying between the extremes of actual or potential cautery ; and emollient poultices and fomentations. The selection and adaptation of these must be left to the judgment of the practitioner, and to the exigency of the case. The prescription of a general rule may perhaps be safely admitted, that whatever application aggravates pain, is improper, and this will sufficiently guard us against the abuse of such stimulants, as under the notion of preventing or impeding mortification have been too frequently employed. It has been already observed that *excessive* inflammation usually precedes destruction of the diseased parts, and in eight cases of carbuncle which have fallen under my notice, this was invariably true. *Pain, heat, and rubescence*, determine the degree of this excess ; and whatever is found most effectually to suppress these,

without danger to the life of the part, will be the best remedy in that particular case. The superior advantage of cold applications, and in what degree they may be safely and most successfully used, farther experience must decide. Water seems only preferable to other fluids, as an inert medium, easily procured ; which transmits heat, and evaporates readily. I have not tried it in any case of extensive inflammation colder than forty-five degrees of fahrenheit's thermometer.

The most immediate and obvious effect is diminution or suspension of pain. A cambric handkerchief, folded into six or eight doubles, and dipped in spring water, was applied over the inflamed part, and renewed as the sensations of the patient determined, at intervals ; varying at different periods of the disease, from five minutes to half an hour. Sometimes the water was poured in a quick succession of drops, or in a capillary stream, over the surface of the inflamed part, and from this mode he seemed to derive the most perfect exemption from pain.

On the 11th August, the heat of the integuments was 106° , but its rapid evolution and diffusion, impressed the sense of a much higher degree. The hand, when swept gently over at the distance of three or four inches from the surface, received a sensation similar to that which a heated andiron would have given at the same distance, and water by flowing over the tumor in drops, as before described, acquired fifteen degrees of heat. The compresses were warmed so much as to be no longer useful in four or five minutes, and if by accident the regular succession of these was interrupted, pain, heat, and redness, certainly increased. Sometimes the applications were unremittingly renewed during a certain time, i. e. till the pain had nearly ceased; and this point seemed to limit their efficacy, for beyond it, cold produced an uneasy sensation, which always induced the patient to suspend the proceeding. The utility of cold was particularly evident—1st. Upon the 6th of August, when substituted for the poultices—2d. Upon the 16th and 20th after being discontinued for

a short time, and 3d. upon the 6th and 7th of September, in arresting the progress of the new carbuncle; but during six weeks, very few days elapsed without some decisive proofs of its efficacy.

The secondary tumors or buboes are usual consequences of carbuncle.—Mr. Bromfeild ascribes them to injury of the membranes, which he observes are often destroyed beyond the extent of the inflamed integuments; admitting this explanation, it may be asked why these pustules are not distributed more equally around the circumference of the tumour but arise in regular progressive descent as before described? has not absorption of the pus from the surface of the back, down which it flows, or the gradual diffusion of it through the cells of the subjacent membrane some influence in their production? however this may be, I am sorry to add, that none of the various means used, seemed to prevent these minor carbuncles, which ultimately proved hardly less troublesome than their principal.

One circumstance more alone seems to merit attention ; viz : the advantage of a decumbent posture in every period of the disease, owing probably to muscular relaxation, but the patient, whose uncommon fortitude enabled him generally to regulate the measures by his own experience, could not fully avail himself of this observation.

ADDITION BY THE EDITOR.

A single glance will nearly suffice to satisfy the philosophical observer that there has been long wanting to medicine a great body of information *concerning the treatment of diseases by temperature*. Convinced of the practicability of supplying this desideratum, many years ago I pressed different medical friends to avail themselves of various well-known analogies.

In the above-mentioned case of the person of 70 years of age, the application of cold was, in part at least, occasioned by such a conver-

fation. And I well remember bearing daily of the happy effect of the expedient. When it was neglected by the attendants (as sometimes happened,) the pain and inflammation returned with their original violence. Hence the patient's friends were rendered doubly diligent : and they had soon to congratulate him upon his escape from a danger, from which at that age no other plan would probably have rescued him ; not to mention the dreadful pain, preceding mortification, of which he endured but little.

The idea of regulating extensive as well as limited, morbid actions, by temperature, (though never pursued as far as it ought to be) has surely occurred to thousands of practitioners. In the particular case of carbuncle, cold applications have been suggested ; among others, if I mistake not, by Mr. Rigby in his publication on animal heat. Still however this practice undoubtedly requires to be enforced. In Ploucquet's laborious digest of medical facts, *Initia bibilothecae medico-practicae, et chirurgicae realis,*

the method is not even hinted, under the heads *anthrax*, *carbo*, *carbunculus*, *ignis perficus*, *pruna*.

Nay more ; I have good cause to know that there are in this country situations, where a dangerous attack of carbuncle would place the surgeon between the alternatives of hurtful routine practice at the hazard of the patient's life, or of rational practice at the hazard of his own reputation. Among the correspondents whom I have induced to apply cold water or ice during the inflammatory stage, the very case has occurred. From one of these I have a letter before me, admirably delineating at a single stroke that class of medical men, who having acquired vogue by qualities entirely foreign to their profession, press with the whole weight of their authority upon the art itself, lest it should advance and leave them behind.

The patient mentioned by my correspondent, had received almost entire ease from cold water ; and the inflammation seemed to be held

in check, when from the anxiety of friends, another (fashionable practitioner) was called in. "His disapprobation of our practice," says the account, "was immediately manifested by the preference he expressed for the old plan of treatment by emollient applications and poultices; *mingled with affected admiration of the courage which had pushed to such an extent, the trial of a new remedy.*"*

From part of the following sentence may one not suppose that the eye of a keen observer had been caught by those members of the medical fraternity that without reflection or remorse, go on eternally repeating the same vain processes of their art; and never let slip

* I ought to add, that the discontinuance of the compresses wetted with cold water, in the case before related at length, arose from a scruple expressed by a senior surgeon of great respectability; so little is the method understood. This circumstance has been suppressed by the reporter out of delicacy; but it ought to be mentioned; otherwise the treatment would appear capricious. I need not say how strongly the variation confirms the doctrine deducible from the case.

an occasion of blasting by insinuation the character of a rival, who to assuage pain, or preserve life, shall dare depart from precedent ? Est-il une fin plus triste (says Rousseau, lettre à M. de Voltaire 18 Août 1756), *que celle d'un mourant. . . . que les medecins assassinent dans son lit à leur aise*, et que des prêtres barbares font avec art favoriser la mort ?

Left the foregoing facts or my remarks should mislead, it should be added, that free application of cold seems only admissible in genuine primary inflammations, threatening to end in suppuration, extreme debility, or gangrene. In secondary affections, not attended by strong pulse, and existing in feeble constitutions, such treatment should be cautiously pursued. The following case will illustrate the rule.

A lady had swelling, pain, and heat, in one cheek immediately after tooth-ache. On the second day no abatement. I advised her to put wet linnen to the cheek. No effect followed in a quarter of an hour. The pulse being weak

and not frequent, I did not push the cold application. Next day, towards evening, the pain and heat were intense; the swelling larger, with redness. The throat was a little sore. At intervals a pain was felt to dart from the bottom of the sternum to the spine. The pulse was not stronger or more frequent than in health.

Four folds of filtering paper, dipped in pump-water mixed with a little spirit, were laid on the cheek, and very frequently renewed for above half an hour. At first some ease seemed to follow. But the patient soon began to complain of an increase of the darting pain; and at last distinctly observed for a number of times in succession that every renewal of the cold application brought it on with violence. At the same time she remarked that the pit of the stomach was drawn inwards. Hence (the pulse being under 80, and weak) I judged the darting pain in the chest to arise from irregular contraction of the diaphragm, and not from inflammation: and as this pain had now become

excessive, superseding all sensation from the cheek, I desired that the cold application might be discontinued, and a grain of opium taken. This was done, and the pain in the chest soon subsided. The patient slept well (which had not been the case for two nights before) and found the cheek much reduced in the morning.

We often hear of the danger of exposure in the measles. I knew an instance where a delicate woman, towards the close of the complaint, had spasmodic seizures on the admission of cold air into her apartment. I suppose, in conformity to the rule above laid down, that this happened from inattention to the stage of the disorder, and that it would not have happened during the inflammatory period, while the diseased actions were strong.

The case of Miss Brown, with others of the same kind, will hardly be thought remarkable since Dr. Currie's able treatise on cold ablution in fever. It was briefly this. She had several

days before I saw her been affected with pneumonia. Her strength was so reduced as to raise apprehensions (to which I should not probably now yield) respecting further evacuation. I then recommended only that she should be kept in a room without a fire, and that a mixture of water with spirit should be frequently applied to the trunk; the extremities being covered sufficiently not to feel cold. The succeeding day the heat of the skin, and the distress of respiration were removed, and she grew gradually well.

The following case shews the danger of inadvertence in not keeping up attention to temperature, and would have appeared to the late Dr. Brown a valuable fact. Master Y. four years old. Dec. 29, 1793; has a dry hard cough—some pain in his chest; pulse 140. Face flushed—skin exceedingly hot—tongue covered with a thick white crust—eyes very heavy—eyelids puffed. Had an antimonial emetic last night.

As his chest is very narrow, and his habit feeble, bleeding is strongly opposed, though it ought undoubtedly to be practised.

R Mistur. salinos. ℥i

Antimon. tartarizat. gr. $\frac{1}{4}$ M Capt.

secundâ quâque horâ.

At night rather better. 30th in the morning, symptoms little abated: medicine continued—ordered to be kept in a room without fire; but the extremities not to be suffered to grow chill. At night breathing easier, cough less; skin cooler—pulse only 92: two loose stools. Discontinue the medicine. Dec. 31st. Had slept with heavy bed clothes—towards morning appears very ill. At eight o'clock pulse 140, weak. Cough frequent, with dyspnœa, but looser, with secretion of mucus which he does not spit out. Tongue foul, browner. Qu. Is not the cause of this violent exacerbation his not being kept cool in the night as well as the day? Directed to be taken out of bed and kept cool. Eleven o'clock

A. M. Pulse has fallen to 120, and is stronger—less cough—no laborious respiration—no pain in the side—skin cool—no appetite. One o'clock P. M. Had risen. Pulse 108—skin cooler—tongue moister. Eight o'clock. Sleeping *upon* the bed—sleep composed—body of a natural temperature to the touch. A glyster had been injected, and had opened the body. To night to have less bed-clothes, and to be stripped or taken up if he grows hot. Jan. 1st. Has had a good night—appetite better—cough loose. From this time he amended, and by degrees perfectly recovered.

To Dr. Hamilton of Ipswich who, under the disadvantage of blindness, has improved his treatise on hydrophobia into the best repository of facts extant upon this subject (*Remarks on hydrophobia. Longman. 1798*), I am indebted for a very interesting fact concerning the operation of cold in inflammatory affections of the chest.

*Instance of CATARRH cured by exposure to open
air in a frosty night.*

The following anecdote may be worth your notice. It appeared to me so uncommon that I had it written down at the time it happened which was on the night of the 27th of February 1797, and succeeding morning. Our weather, from the beginning of Feb. was extremely variable as to heat and cold, ever running into extremes for the season, but steady with respect to the absence of rain, and accompanied with a clear sky. My thermometers are six feet from the ground, in an arbour in the garden, shaded from the sun, and almost from the wind. The arbour fronts the north, and is surrounded by high walls on one side, and houses on the other, at about thirty yards distance. On Feb. 19th, at 7 A. M. the mercury stood at 25°.— 3 P. M. at 50—12 P. M. at 30—

Feb. 20—7 A. M.—at $22\frac{1}{2}$ —3 P. M.—at 49—12 P. M.—at 28
 —21—7 A. M.—at 25—3 P. M.—at 48—12 P. M.—at 30
 —22—7 A. M.—at 26—3 P. M.—at $53\frac{1}{2}$ —12 P. M.—at 30
 —23—7 A. M.—at 27—3 P. M.—at 54—12 P. M.—at 32
 —24—7 A. M.—at 27—3 P. M.—at $57\frac{1}{2}$ —12 P. M.—at 31
 —25—7 A. M.—at 31—3 P. M.—at 54—12 P. M.—at $36\frac{1}{2}$
 —26—7 A. M.—at $34\frac{1}{2}$ —3 P. M.—at 47—12 P. M.—at —
 —27—7 A. M.—at $31\frac{1}{2}$ —3 P. M.—at 44— — —
 —28—7 A. M.—at 22—3 P. M.—at $40\frac{1}{2}$ —12 P. M.—at 32

The first week of March it did not descend below 32° except one morning, though it kept pretty near it. The middle of the day was warm, and without rain, as was the case till the equinox.

This little sketch of our weather will account for the universality of catarrhal affections among us, accompanied with more inflammation than for the most part happens in common colds. In many cases they were truly pneumonic, and proved suddenly fatal, especially to elderly people. The fineness of the day caused both the morning spencer and the evening furtout to be neglected; and in eight hours, as you see, they were carried through a range of temperature sometimes exceeding 35 degrees, beginning with 10 degrees of frost, and in the

succeeding eight hours, plunged back into a degree of cold not much less than that of the morning.

On the 27th of February, a little boy who has lived with me in the capacity of a servant for about a year past, and is my guide when I walk out, and reads and occasionally writes for me when at home, was sent between three and four P. M. with a newspaper to the next street. He had been affected with this complaint for about ten days, as all the family were, and his cough with febrile heat was considerable, especially the former. He is about ten years old ; is delicate and even small of his age, and apt from a playful disposition and lively turn, to run the rig with the boys in the street, in place of going on my errand when I send him out, sometimes not returning for two, three, or more hours. As he is useful to me, and an orphan left to the care of the parish, I indulge him considerably, besides being his preceptor. When six struck, John was not returned ; eight came, no John appeared ; nine, and no

discovery made of him. The night, as you perceive, was very cold ; his complaint somewhat alarming, and I under considerable uneasiness left it should increase from his being heated, as I presumed, by play, and suddenly cooled by the frost. When eleven came, the messengers I sent returned without finding him, but with the intelligence of his having been seen at play about twilight. It was in vain to search farther at this late hour, and I concluded he must have gone with some playfellow for the night, for fear of chastisement for his neglect. When morning appeared, as soon as persons could be got, I sent again in search of him, but it was eleven o'clock before he was discovered, running out of town with a croud of boys after the soldiers who were about to be reviewed. On interrogating him at his return respecting his absence, he confessed fear of punishment prevented his return, from a consciousness of misconduct. He positively asserted he had been in no house since the time he left me till his return now ; that he had walked the street the whole night, a little time excepted, when he

fat down on a stone stair leading to a shop, to rest, but did not sleep; that when day appeared he repaired to the barracks, and on the gates being opened, went in and walked about till parade hour. He followed the soldiers to roll-call, and thence to the field, on the way to which he was discovered. The mud on his great coat, and his dishevelled hair, bespoke he had not been in bed. The night, as you have seen, was one of the coldest we experienced since December, and even then exceeded by one or two nights only;* for at 7 A. M. during his perambulation, there was 10 degrees of frost, and at five, which I presume might be the coldest part of the morning, we may fairly

* December 23, 1796, Mercury stood at 26°—At 8 A. M. next morning, at the same hour it came down to 16—At 8 P. M. of same day it stood at 15.—An hour after it came down so low as 10.—At 7 in the morning, Dec. 25, it had sunk to 2. two feet from the surface of the earth. At 10 A. M. it had ascended to 12—At 4 P. M. it was 21½ At 6 P. M. it stood at 23—At 8 P. M. it sunk again to 21. Two hours after it rose to 28. and next morning at 8 it was at 31.—consequently, the night this boy walked the streets was colder throughout than any night of this remarkably cold month, one only excepted.

allow the air to be cooled still two degrees more. John's story was artless and simple, and I could not doubt the truth of his relation. Instead of punishment, which I thought he had severely felt already, he received my sympathy. During the interrogation he underwent, and which took up a considerable time, I observed he did not once cough although in the same space of time since the commencement of his catarrh, it would have often harraffed and interrupted his narrative.

Twenty hours had now elapsed, without food or drink, and exposed to an atmosphere of great cold, and yet he seemed nothing affected, except from the temporary fear of the rod.

I watched him narrowly, as I apprehended a fever might be the consequence of his midnight ramble; but here I was agreeably disappointed, for in place of fever his catarrh was cured, his cough ceased, and never after returned. He had some food given him, but it was sparingly,

and simple of its kind, being a little plain peas-soup, with some fish and potatoes ; and I restrained him from the warmth of the fire, by ordering to an out-house to clean some knives, judging it unsafe for him to remain in a heated room in his present state of accumulated irritability ; and even when night came I was under apprehension from the effect of a warm bed. I had some years ago seen the bad effects of sudden warmth succeeding cold ; or in other words, warm cloathing exchanged for rags and nakedness. It was the case of Watson, published in my Reg. Surg. Ed. 2d. 1794, a vagrant who had been a drummer in the regiment where I served, and who found me out here in the course of his ramblings. I cloathed him completely. The waistcoat was composed of warm materials, lined with flannel, which I used to wear on horseback through the winter. The day he put them on was a mixture of rain, snow, and sleet, which had buffeted his bare body in several parts. Next day he was extremely ill, with catarrh and fever ; and the succeeding day his complaints increasing, he threw off the

warm cloathes, and betook him again to his rags, swearing the warm cloathes had killed him.

With respect to the cure performed on my boy, I shall leave it to you to supply explanatory reflections. We cannot send our patients, when they apply to us under this complaint, to walk abroad in frosty weather; but we can forbid them their warm drinks, warm rooms, increased quantity of bed-cloathes, &c. &c.

R. H.

In some of our older medical authors, there may be found on the subject of temperature, very valuable collections of facts of which we have not yet fully availed ourselves. The facts themselves can hardly be rejected as fabulous. They are too numerous, distinct, independent of speculation, and (as far as the comparison can be made) consonant with our latest and most accurate observations. Dr. Edward Baynard,

who honestly owns them, to “ be past of his philosophy,” gives the following relation. . It is the most interesting of the kind I have met with, and may in many dangerous cases (particularly of the natural small pox) be useful to modern practitioners.

“ In *fevers* I have known a great many in my time, who by the over-care of their *health-wrights* were made *delirious*, and in their phrensy have leaped into a *pond*, or any other *cold water*, and not *one* as ever I heard of got any harm, but were thereby presently cured. And Dr. *Willis*, I remember, instances a case or two, wherein they have recovered by immersing into *cold water*, either by accident or distraction. And lately I saw at Mr. *Charles Frusbaw's* in *Salisbury Court*, a servant maid, who not long before being delirious in a most intense fever, got loose and leaped into the river *Thames*, but being soon taken up by a boat, was brought home in her wet cloathes, who no sooner being stript and dry cloathes put on, but she went about her business, and was as well as ever she

was in her life. I had often heard this story in the neighbourhood, but being curious in the thing, I sent for the maid, and had this relation from her own mouth.

“ A learned and ingenious gentleman, a doctor of laws, now living told me, that being light-headed in a fever, and most intensely hot and thirsty, got from his nurse, and rushed into a horse-pond in the yard, and there layed above half an hour ; it brought him presently to his senses, and allayed both his heat and thirst. After which, when in bed, he fell into a sound sleep, and when he awaked (in a great sweat) he found he was well, but complained of a great pain in his head for some time after, which he himself thinks proceeded from not wetting his head.

“ Mr. Carr, the present schoolmaster of Marlborough, told me, that he recovered when given over in a fever, by drinking a large quantity of cold spring water. And that I have known in twenty such cases in my time, but

that is not to be depended upon, for some have also recovered by a quite contrary method, as drinking strong fermented liquors, as cyder, sack, claret, &c. in large quantities. See *Harmanus vander Heyden de usu aq. fontanæ & seri Lactis.*

“ A Turk (a servant to a gentleman) falling sick of a fever, some one of the tribe of *treacle-conners* being called in, whether Apothecary or Physician, I cannot tell, but, (according to custom) what between blister and bolus, they soon made him mad. A countryman of his, that came to visit him, seeing him in that *broiling* condition, said nothing, but in the night-time by some confederate help got him down to the Thames-side, and soundly ducked him. The fellow came home sensible, and went to bed, and the next day he was perfectly well. This story was attested to me by two or three gentlemen of undoubted integrity and worth ; and I doubt it not, but believe it from the greater probability ; for I will hold ten to one on the Thames-side against treacle, snake-

root, &c. and that all hot regimen, which inflames and exalts the blood, breaks its globules, and destroys the man, and then forsooth the doctor sneaks away like a dog that has lost his tail, and cries it was a pestilential malignant fever, that no body could cure, and so shews his care of the remainder, bids them open the windows, air the bed-cloaths, and perfume the room for fear of infection, &c. And if he be of the right whining, canting, prick eared stamp, concludes as they do at Tyburn, with a mournful ditty, a psalm, or a preservative prayer for the rest of the family, &c. so exit Prig, with his starched formal chops, ebony cane, fringed gloves, &c.

“Dr. Yarborough told me, that his kinsman, Sir Thomas Yarborough, sent him a letter from Rome, wherein he gave him an account of a footman of his, who when delirious in the small-pox, got from his bed, and in his shirt run into a grotto of a Cardinal's, where there was water, in which he plunged himself, but was presently got out : the small pox seemed to

be sunk and struck in, but upon his going to bed they came out very kindly, and he safely recovered.

“ But my learned and worthy friend Dr. Cole, shewed me an account from an Apothecary in Worcestershire, whose name (I think) was Mr. Matthews; the substance of which was, that a young man delirious in the small pox, when his nurse was asleep, jumped out of bed, run down stairs, and went into a pond; the noise awaked the nurse, who followed with an outcry, which outcry raised the posse of the family, who surrounded the pond; but he parled with them, and told them, that if any body came in he would certainly drown them, and that he would come out when he saw his own time; and accordingly did so, and walked up stairs, and sat (in his wet shirt) upon a chest by the bed-side; in which posture Mr. Matthews found him when he came into the chamber. Note here, that the Apothecary lived three or four miles from the place, and he was in the water and on the chest all that while in his wet

shirt, that the messenger was gone for him. This apothecary, Mr. Matthews (for so I take his name) asked him, how he did? He answered, pretty well. He asked him, if he would have a clean shirt, and go into bed? He said, by and by he would; which accordingly he did. When in bed, he asked the apothecary, if he had nothing good in his pocket, for he was a little faintish? He said that he had a cordial, of which he drank a good draught, so went to sleep, and awaked very well, and in a little time recovered. Now, as Dr. Cole observed very well, a man, quoth he, would not advise his patients in such a case to go into cold water, though this man escaped without injury; but it gives a good occasion to reflect on the many mischiefs that attend the small pox in the hot regimen, since such extravagant and intense cold does so little or no harm.

“ Dr. Dover of Bristol, told me of a Vintner's drawer in Oxford, that in the small pox went into a great tub of water, and there sat, at least

two hours, and yet the fellow recovered, and did well.

“ A gentleman delirious in the small pox, run in his shirt in the snow, at least a mile, and knocked them up in the house where he went, they being all in bed ; the small pox sunk, yet by the benefit of a looseness he recovered.

“ I remember about two years since, a learned gentleman, a divine, told me, that in the country where he was beneficed, in a small town, not far from him, many died of a malignant small pox. A certain boy, a farmer's son, was seized with a pain in his head and back, vomited, was feverish, &c. and had all the symptoms of the small pox. This youth had promised some of his comrades to go a swimming with them that day, which notwithstanding his illness, he was resolved to go, and did so, but never heard more of his small pox. Within three or four days, the father was seized just as the son was, and he was resolved to take Jack's remedy ; his wife dissuaded him from it,

but he was resolved upon it, and did immerge in cold water, and was after it very well. The worthy gentleman that told me this story, promised to give me it in writing, with the persons names and place; but I neglecting of it, he went out of town in two or three days, so I lost the opportunity of being better informed.

“ Mr. Lambert, brother to my worthy friend, Mr. Edmond Lambert of Boyton, in the county of Wilts, told me, that when he was at school in Dorsetshire, at least thirty or more of the boys, one after another, fell sick of the small pox, and that the nurse gave them nothing else but milk and apples in the whole course, and they all recovered. There was but one dissenting boy from that method, who by command from his parents, went another course, and he had like to have died; nay, with very great difficulty they saved his life. And since, another gentleman told me, that himself and divers others were cured by milk and apples, and buttered apples, in the worst sort of small pox.

“ I was at Chiswick, and sometimes in London, in the time of the great plague, in the year 1665 ; and I very well remember, that it was the talk of the town, that a brewer’s servant at Horsleydown in Southwark, was seized with it, and in his delirium run into a horse-pond, first drank his fill, and then fell fast asleep with his head upon the pond’s brink, where he was found in the morning ; how long he had been in the pond, no body knew, for it was in the night he went into the water, and had no nurse then with him, but he recovered to a miracle.

“ I heard also about that time of a nurse taken with the plague, that accidentally fell into a well, somewhere near Acton, and was immediately brought to her senses and recovered. I was told this by some Acton men.”

Floyer and Baynard’s psychologia, p. 226—232.

In respect to the cold bath in the yellow fever, though, as Mr. Yonge says, it did not come early into question at Philadelphia, the physi-

cians at New York employed it much, as we find in various works, as Hofack's Essay, New York, 1797, and the medical repository, No. 2, p. 233 ; where it is said that cold water, thrown on the patient in the first days of fever, reduced the pulse in frequency and force, diffused a general and pleasant coolness, and mitigated or removed pain. These changes lasted in proportion to the extent to which the cold affusion was carried, and to some other circumstances. Afterwards the heat, pain, and fever returned, so as to become worse than at first. Towards the close of the disease, cold affusions are said never to have occasioned more than a slight and temporary return of warmth. " More
 " often the skin has continued cold ; and we
 " have been certain that such a torpor of the
 " system has been induced, as has aided the
 " progress of the disease. On the contrary,
 " when (in the first days of the fever) while the
 " heat was great, the pulse strong, and the
 " strength of the body not yet reduced, we
 " have noticed, that a gradual application of
 " cool, or even cold water, so that the whole

“ body should be washed, has had very happy
 “ effects, especially *if persevered in and fre-*
 “ *quently repeated.* Like beneficial conse-
 “ quences result from similar applications,
 “ similarly conducted, towards the close of the
 “ disease, if the morbid heat of the skin con-
 “ tinue. Nor is it of any great importance
 “ whether the water be cold or tepid, if the
 “ patient be freely exposed to the air. . . Thus
 “ the strong pulse of the first stage is diminished
 “ in force; the weak pulse of the last in fre-
 “ quency, and perspiration or sweating follows.
 “ That this is the fact, whether the fever be
 “ inflammatory or putrid (to use the ordinary
 “ terms of medical writers) is confirmed by the
 “ experience of those who have written from a
 “ careful attention to facts happening under
 “ their own eyes.”

In the scarlatina, I have known excellent
 effects from free exposure of the body to the cool
 atmospheric air, and in this disease have recom-
 mended it to lay children upon single linen,
 stretched and supported by treffels. In the

croup I have kept children with decisive advantage in cold air. Here also I doubt not but constant cold applications to the throat would do service.

But what would save multitudes (particularly of children, who perish because neither parents nor medical men, such as are generally first called to them, have any proper conception of the effect of temperature on their disorders) is an apparatus for applying heat or cold at pleasure to the whole body or any part of it.

To persons subject to cold feet, I have long been accustomed to recommend, in the day time, a tin vessel capable of holding their feet, and having double sides, the interstices between which are filled with hot water. I am informed, that *footwarmers* of a more convenient construction than mine are now made by Lloyd, near Norfolk-street, Strand, London.

MISCELLANEOUS REPORTS AND OBSERVATIONS

Concerning the respiration of
GASES AND VAPOURS.

Since the publication of the last part of CONSIDERATIONS ON THE MEDICINAL POWERS OF FACTITIOUS AIRS, various reports, more or less favourable, have been transmitted to the editor; and he has been offered communications which would have filled two or three volumes. He however thought it right to decline these obliging offers, and to forbear publishing any thing, till the establishment for the investigation of the powers of these substances should precisely ascertain facts. In the CONSIDERATIONS, enough was stated to exemplify the practicability of the occasional use of small quantities of modified air: and some of the reports convincingly prove the efficacy of the treatment. See in particular the case of Mr. Atwood, p. 57, Part i. Edition 3d. and other similar cures of sore legs. But in most instances, further and more elaborate researches are undoubtedly wanting to determine what share the gases had in the favourable result—and the editor's view in publishing them was to encourage the trial of these means where all others are avowedly unavailing.

The three following communications are selected from among many others; and the subjoined sentiments of foreign physicians cannot but be acceptable to the curious English reader.

Letter from MR. CREASER, Surgeon, Bath.

Dear Sir,

The disease which I am about to communicate to you the treatment and event of, appears to me to have been so anomalous as not to have been exactly described by any nosologist, though it resembles most the stomacace of Sauvages, Order 5, genus 27, species 2. The subject of it was a young married lady who had been generally chlorotic, and in whom the discharge, when it occurred, was remarkably small and ferous. After marriage she became pregnant several times, but always miscarried, and a perceptible degree of chlorosis was constantly manifest with its usual symptoms.

Some years ago a slight herpes appeared on the face, in the vicinity of which effusions of blood took place into the cellular membrane, with enlargement of the capillary vessels. Those effused parts generally discharged themselves by

bleeding externally, and so frequently that I have known thirty ounces of blood lost by this way in a week, and seldom less than six or seven ounces. I believe that the hæmorrhages which occur as a relief to the other diseased actions, very frequently exceed the bounds of what is necessary, and become in themselves diseases. This is often the case in hæmorrhoids and bleedings from the nose.

I considered this disease as dependent on the deficient menstrual discharge, and administered a variety of the uterine stimulants, as they are called, without effect. Steel constantly improved the general vigor of the system, and as uniformly in proportion increased the hæmorrhage from the capillaries of the face. The usual systems of treatment by mercury and other remedies, as well as the most astringent and stimulant topical applications, had also been inefficaciously employed.

Under these circumstances, and when the discharges were both frequent and considerable,

I began to administer oxygen gas in the accustomed way. Soon after its administration, the symptoms dependent on the chlorotic state of the system were considerably amended. The difficulty of breathing, and oedema of the legs, which had subsisted in a considerable degree, were nearly removed, and more perfect sleep was induced. After its use for a month, by which time three quarts of oxygen were given three times a day, a more healthy colour of the skin succeeded, and the livid colour and vascular appearance of the effused parts began to disappear. The hæmorrhage was so much diminished, that it was thought necessary to insert an issue for the purpose of obviating the effects of the cessation of an accustomed discharge of such magnitude.—This was the only additional remedy employed; and lest any improper stress may be laid on its auxiliary effect, I must observe that it was not adopted till after the amendment was considerable, and that it had before been employed singly without any effect.

The oxygen was continued in the same

quantity for three months, with progressive amendment, but not with entire cessation of the discharge. It was then omitted, from a supposition that it might lose its power from habit ; but after the intermission of its use the improved health of the system continued, and the disease gradually disappeared.

The singular conclusions which appear to me to be deducible from this case are these.—The oxygen increased the fulness, frequency, and strength of arterial action, and the heat of the body, but did not increase the discharge, as steel, exercise, and a full living did. We must either, therefore, explain the effects of oxygen by the opinion of the late Mr. Hunter, that action and strength are essentially different, and that it is one of the few stimuli which confer both ; or in the opinion of some more recent physiologists, we may suppose that oxygen communicates irritability to the system, and thus affords a source of excitement. This appears to be confirmed by oxygen's rendering the body more susceptible of other stimuli, as

purgatives, of which some facts are in my possession. I remain, dear sir,

Your obliged and respectful servant,

THOMAS CREASER.

Dr. BEDDOES.

Letter from Dr. CROWTHER.

WAKEFIELD, 1798.

To Dr. BEDDOES,

Sir,

It gives me great pleasure to be able to add one to the many favourable testimonies which you have published, respecting the efficacy of pneumatic medicine. Nothing can be more decisive than the curative effect of hydro-carbonate gas, in the subsequent case of Mr. Orange.

Much has been said by the enemies to innovation in medicine, respecting the inaccuracy and misstatement of some of the cases which have been sent to you. It has been insinuated, that some of them have been fabricated by young practitioners with a view of bringing themselves into notice. That no such implication may be attached to the present case, I hereby pledge myself to introduce to the pa-

tient, any neighbouring medical practitioner into whose hands the case may fall, who being a sceptic respecting the efficacy of factitious airs, may wish for further testimony on the subject.

I was called to see Mr. Orange, after a veteran in physic had declared the case to be consumptive and incurable. When I was interrogated by his friends respecting the prognosis, I answered, that according to the usual mode of treatment, the prospect of recovery was very unfavourable; but that some cures had lately been performed in similar cases, by the use of factitious airs. The success far surpassed my most sanguine expectations.

Case of Pulmonary Abscess.

Oct. 28th 1797, Mr. Joseph Orange, grocer, Wakefield, a married man, aged 26; is affected almost incessantly with cough and copious expectoration. Last night he began to spit up large quantities of thick, foetid, brownish,

purulent matter, which sunk in water, and rendered the room in which he sat very offensive : his pulse is about a hundred in a minute, and of moderate strength ; he is affected with hectic heats in the afternoon, and sweating in the morning. Bowels rather loose, countenance pallid, appetite and strength much impaired, tongue foul. He is much emaciated, his eyes appear hollow, and his cheek bones very prominent. He has been confined to his room about a fortnight, but has expectorated mucus streaked with blood, and been affected with cough for some time past. He has been subject to cough, pain in his sides, and dyspnoea at times, for several years.

He was unusually dejected and oppressed last night about the precordia, before the purulent expectoration commenced. I ordered a mucilaginous mixture for the cough, and a bolus to be taken at bed time, with a grain of opium, a grain of balsam of tolu, and five grains of alum ; and directed him to inhale twice a day, a pint of hydro-carbonate gas,

mixed with twenty-four quarts of atmospheric air.

29th. Slept better last night than he has done for some time past; felt no vertigo from the hydro-carbonate gas, but thought himself refreshed by it. Cough less frequent. Ordered him to continue his former medicines, to take five grains of ipecacuanha in the morning, and to live principally upon good milk.

30th Was again much dejected and oppressed about the præcordia last night, which as before, was followed by a very copious expectoration of purulent matter during the night. He slept ill on account of the cough; the emetic operated gently. To continue his medicines.

31st. Had a tolerable night, but sweated profusely this morning; cough less troublesome; expectoration somewhat diminished. Says that he always finds himself refreshed after breathing the reduced atmosphere, and feels disappointed

and uneasy when it is not administered to him at the usual hours. I ordered him to increase the hydro-carbonate to a pint and half for a dose, to rise out of bed when the sweats come on, and take twenty drops of elixir of vitriol in water. I directed him to have an issue in his side capable of containing four peas, and to use exercise frequently in a swing erected in his sitting room for that purpose.

5th Nov. Cough, expectoration, and sweating, are much diminished ; he uniformly finds himself refreshed after breathing the hydro-carbonate air, which has been increased to a quart for each dose. Pulse 94. I have the satisfaction to find that my directions have been implicitly complied with. Ordered him to continue his former medicines, and to take a powder twice a day, containing gum myrrh eight grains, vitriolated iron one grain and half, powder of colombo root ten grains.

10th. He continues gradually to recover ; appetite much improved ; dyspnœa and sweat-

ings nearly gone ; cough much less troublesome ; expectoration much diminished. Ordered him to continue his medicines, and to take two pints and half of hydro-carbonate for a dose.

17th. He has recovered very rapidly since last report ; his cough and expectoration are nearly gone ; he gains flesh and strength daily ; his appetite is greater than usual when in health. After running up two pair of stairs, his pulse beat only eighty in a minute. He breathes more easily than he has done for several years before.

Dec. 25th. Mr. Orange is now in perfect health.

Jan. 3d 1798. By acting in every respect contrary to the directions which I had given him, from the idea that he was so well that nothing could possibly hurt him, Mr. Orange has brought on a return of his cough with copious expectoration. He has exposed himself

without proper covering to the inclemency of the weather, and has enjoyed at late hours, and in crowded rooms, the convivial meetings usual at this season of the year. By a recurrence, however, to his former mode of treatment, in the course of about a fortnight, his health was completely re-established, and he now remains perfectly well.

Feb. 14th 1798.

Case of Phthisis Pulmonalis, or as some authors would denominate it, Atrophia Lactantium.

May 2d 1797.

CATHARINE GREY, a dispensary patient, aged about 36 years ; was attacked three weeks ago with flying pains in the chest, and cough, attended with copious expectoration of frothy mucus ; her tongue was foul, she had some thirst, her pulse was 110 in a minute ; her appetite was much impaired, and she had regular hectic exacerbatons in the afternoon. About two months ago she was delivered of twins, which she has since nursed, though she

takes but little food, and that of a very coarse kind, being extremely poor.

She has had a blister applied to the sternum, which occasioned a profuse discharge and great debility, with very little relief to the symptoms. I directed her to live principally upon a milk diet, to dissolve one dram of lozenges, containing one grain of opium, in her mouth at divided intervals daily: to take a scruple of peruvian bark, mixed with a scruple of colombo root, thrice a day, and to inhale one dram of the vapor of ether, in an ounce of which half a dram of the dried leaves of cicuta had been infused thrice a day.

12th. She appears to have lost strength considerably since last report; her cough is very troublesome, and she expectorates large quantities of mucus streaked with blood. In a morning the matter expectorated is heavy, at other times light and frothy. The bark disagrees with her stomach, but she imagines that she finds considerable relief from the vapor of the

etherial tincture of cicuta. I was not acquainted until this day, that she suckled her twin children. She is ordered to wean them immediately, to omit the bark, and take instead thereof, one ounce of a mixture composed of decoction of bark and bitter infusion, every three hours, and an opiate at bed-time. The vapor of ether to be continued. She will now be regularly supplied with as much good milk as she can use.

23d. She imagines that she finds relief from the ether, but is evidently weaker since last report. She has been attacked with diarrhœa and colliquative sweats., and expectorates as much as formerly. She is ordered to omit the decoction of bark, to continue the ether and opiate, and to take one ounce of the cretaceous mixture along with one dram of the tincture of catechu after every loose stool.

30th. The diarrhœa has considerably abated ; she feels a good deal of pain in her breast from coughing, in other respects she is much the same ; the appetite is weak. She is ordered to

continue the ether and opiate, to have an issue in her side, and to take the following powder thrice a day ; colombo root one scruple, gum kino and myrrh each eight grains.

June 14th. Her cough is less frequent, her expectoration is diminished in quantity, and she thinks herself stronger : her pulse is still quick, and she continues to perspire much in the night. Ordered to repeat her former medicines, and to take forty drops of diluted vitriolic acid every night at bed-time.

21st. She has recovered rapidly since last report, the colliquative sweats have ceased ; her cough, expectoration, and hectic heats diminish daily. Pulse about 90, appetite and strength much improved, she takes milk very freely. Ordered to continue her medicines.

28. She still recovers rapidly. Ordered to continue the ether, and to take the following powder thrice a day : colombo root half a dram, myrrh eight grains, vitriolated iron one grain mix.

July 2d. On the 30th of June, the catamenia appeared in unusual quantity, and weakened her much; she discharged likewise by coughing, a piece of grumous blood about the size of a hazel nut. Ordered to omit her medicines, and continue the ethereal tincture only.

July 10th.—She continues to recover her flesh and strength very fast, her hectic heats and sweats have left her; her pulse is about 80 in a minute. Her cough is nearly gone. She attributes her recovery entirely to the ethereal tincture of cicuta. By the end of July she was perfectly well, and from a state of extreme macilency became obese. She still continues very well Dec. 10, 1797, since which period I have not seen or heard of her. The disease was undoubtedly brought on by want of the common necessaries of life; she had neither sufficient food nor clothing: the matter of doubt with many will be, whether her recovery ought to be attributed to the nutritive diet which she obtained after I saw her, or to the

vapor of ether and cicuta. I am clearly of opinion that her recovery could not have been effected by either one or the other separately, but is to be attributed to their conjoint effect. I deem it necessary, however, to add, that I have used ether in three other cases not apparently more unfavourable, without permanent advantage. I have used hydrocarbonate air along with diuretics in a case of hydrothorax, complicated with a tuberculous affection of the lungs, with much advantage. I at first gave vital air which aggravated the symptoms. Make what use you please of the contents of this letter.

I am,

Your humble Servant,

CALEB CROWTHER, M. D.

Memorandum from a Friend to Miss NORTON.

As it is only from recollection that an account of Martha Norton's case can be given, I fear it will not be so minute and accurate as Dr. Beddoes may wish.—During last summer (1796) she was frequently indisposed and, every little exertion, even walking up stairs, brought on an oppression of breath and great lassitude.

A journey to London in Sept. was followed by great fatigue and difficulty of breathing, from which she did not recover for some weeks. Early in the winter she had a bad cold and cough, and in January 1797, a cold renewed the cough, which soon became violent and almost incessant, attended by so great oppression of breath that she could not lie down, and intire loss of appetite; the expectoration was great, and sunk in water. She was bled and a blister put on her chest without affording any relief.—Nine days after the appearance of these

symptoms, the Apothecary who attended her acknowledged to her friends, that he thought her case did not admit of hope ; but recommended for their satisfaction, that a Physician should see her ; Dr. Saunders confirmed the Apothecary's opinion, and said nothing could be done but to alleviate the most distressing symptoms. A milk diet and a change of air were prescribed as soon as she could bear a journey, and a medicine given which relieved the spasms (they had been so violent as to make the Apothecary apprehensive of convulsions in which he thought it probable she might suddenly expire) the hectic continued unabated, the night sweats profuse, and the expectoration purulent.

This was her state when the air was first administered. She took twice a day, a quart of the hydrogen diluted with fifteen quarts of common air. For the two or three first days it occasioned a slight degree of dizziness, and depression of spirits. The pulse was immediately softer and less frequent. The expectoration less and likewise better in quality, the

cough gradually lessened ; and ten days after using the air she was intirely free from it, and from every other alarming symptom. Her spirits and appetite returned, and though during the winter and spring she has had frequent colds from the variableness of the weather, they have affected her much less than might have been expected, which is attributed to the constant use of the air, till a fortnight since.

A cold taken in the interval caused a return of cough and hectic, from which she is again relieved by returning to the use of the air.

The Apothecary who attended Martha in London, has been desired to give a more particular state of the case, which as soon as received shall be sent to Mr. K. It is as follows :

Miss M. NORTON of a dark complexion, and a frame so delicate, that mechanical aid was necessary to prevent a distortion of the spine, yet the pupils of the eyes were quick and intelligent without any remarkable dilatation; had been afflicted with stumous inflammation and discharges from the submaxillary glands in a more early period of life. She is at this time in

her eighteenth or nineteenth year ; and was free from any constitutional irregularity or suppression whatever.

About the 20th of January, 1797, was attacked with slight fever and troublesome cough ; on the 28th I first called on her, and found her complaining of great oppression and slight pain in her breast, a cough dry and troublesome, particularly on lying down, with great debility, slight though not constant head ache, considerable heat of skin, want of appetite, and a pulse at 110. An antimonial medicine was given every six hours so as to act as an emetic, and considerably increase the discharge on the skin. On the 29th, a blister was applied to the sternum, and pectorals with salines and an opiate were given ; but without any sensible benefit. On the 30th, about five or six ounces of blood were taken from the arm with no better success, nor could I observe that the strength was impaired by it. Next day but one, Feb. 2, she was visited by Dr. Saunders, who advised her the myrrh draughts in pectoral emulsion every six hours and storax pill at night. These

medicines were continued with little variation or apparent advantage about ten days, the usual mild plan of diet being observed. The expectoration during the whole of this time was in small quantity and in no degree, that I could observe, purulent. The state of the bowels was steady, nor was thirst much complained of. At this time, about a fortnight from my first visit, and three weeks from the first attack, an unexpected and marked remission of fever took place, attended with a considerable and purulent expectoration which seemed, as far as I could judge, to come from a pretty extensive surface, rather than from tubercles or detached spots, or ulcers. This discharge very soon lessened and improved in appearance; in this state of things it was when the hydrogen air was first administered; the fever, quickness of pulse and heat having completely remitted for thirty six hours at least. So that I am really unable to say in this case, whether the air had any share in arresting or resisting the progress of pulmonary consumption with its fever,—a disease, the most miserable, painful and fatal which attacks

the human body, in the part of the world we live in.

The effects of the hydrogen air on Miss Norton, were at first great debility with slight pain in the breast, and great giddiness of the head, though this last symptom is not, I believe, uncommon, to weakly people, in forcibly drawing in common air through any kind of tube. I think myself warranted, however, in saying that Miss Norton recovered more rapidly than any one I had ever seen, in a similar situation, where medicated airs had not been administered. It is further necessary to observe, that Dr. Saunders, as well as myself, thought Miss Norton's case had every appearance of confirmed pulmonary consumption with very little hope of recovery ; yet to some minds where the progress is so rapid, it always leaves hope that the disease is a slow peripneumony, and in this case the mind was influenced and alarmed by the strumous appearances on each side of the neck.

ARTHUR ROBINSON.

37th Southampton Buildings,

May 6th, 1797.

The first person on the continent who in consequence of what I had written, published an experiment with factitious air in consumption of the lungs, was Dr. Girtanner. His patient expectorated matter, had a violent cough with compleat hectic fever and “colliquative bleedings from the nose, which were with difficulty stopped.” The colliquative diarrhœa had come on with violence, and his last physician had declared that he could not hold out above three weeks. April 3d, 1795, he was made to respire one quart of carbonic acid air with two of atmospheric out of a bladder provided with a mouth-piece that covered the mouth and nose, and had two valves opening in opposite directions. The patient said he felt as if a great load was rolled off his breast—he could now breathe more freely, which he was not able to do for a year before. On the right side (which was the painful and diseased part) he felt a gentle warmth and an agreeable titillation. In a quarter of an hour his dose of air was repeated a second, and in half an hour, a third time. Each dose procured agreeable sensations;

the free respiration continued. Pulse small, spasmodic (*krampfhaft*,) 124. Dr. G. willing as he says to follow my plan exactly, made his patient almost entirely abstain from vegetables and live on smoked fish and salted meat. (Here I must observe, that I never proposed either such a diet or the use of factitious airs but as expedients, worthy of trial in so hopeless a disease; by no means as remedies for whose efficacy I could vouch). Next day the patient thanked Dr. G. in the most affecting manner for the good night he had enjoyed. He had slept three hours quietly and had sweated but little. The diarrhœa seemed on the decline, and there had been no bleeding. The respiration of mixed air being repeated as before, produced the same agreeable effects. This plan of diet was persevered in till the sixth of June, and three or four quarts of diluted carbonic acid were repeated twice every day, no medicine whatever being used. The amendment was progressive except during part of May, when the patient suffered from uneasiness of mind. This being removed, the case went on

well. By the 5th of June the patient was got so well that Dr. G. proposed to omit the air. From June 6th to July 8th, his state was variable. His strength however so much improved that in fine weather he could walk two, three or four hours—sleep good, no night sweats, some dry cough chiefly by day. Pulse 90 in the morning—100, 120, in the rest of the day—no flushing—no burning in the palms of the hands.—July 3, he quitted his diet—4th, he had chilliness and coughed all night—9th, he complained of the left side; but the pain was removed by two or three days respiration of the mixed air. An opiate being added, by the middle of August the pulse was natural, and all the symptoms but a very trifling cough gone. On the 27th of August the patient was stout enough to walk twenty (German) miles. At the end of his journey Dr. G. was informed of his being entirely well.

Dr. Hufeland, a writer of much authority in his own country, immediately afterwards published a paper strongly recommending a prosecution of the trials with factitious air, and men-

tioning two or three cases where carbonic acid air directly from an effervescent mixture and vapours from soil carried into the patient's apartment, had procured some mitigation in the last stage of consumption ; and one case in which following the plough had completely and permanently cured confirmed consumption . Seltzer water with milk being the only other means used.

The progress of recovery in Dr. Girtanner's patient having been witnessed by many of the physicians at Gottingen, produced a lively sensation. Further trials were made in Mr. Richter's hospital, and an account at large of these experiments was published in Dr. Muehry's thesis *on the use of the inspiration of fixed air in consumption* Goettingen, 1796. The method of Girtanner, the diet excepted, was tried in five cases of consumption and one of asthma. In the former no recovery was effected : but some mitigation was for the most part experienced for a short time after the inhalation ; sometimes however the contrary effect took place. Little change was observed in the pulse or the

heat. In general no cough followed the operation, except during the exacerbation of fever. In one case the fetid smell of the expectoration was amended. In two cases the night sweats ceased for a time, but recurred. The asthmatic patient was little relieved. The same Dr. Muchry afterwards published another case, in which, beginning with a quart of carbonic acid to two of common air eight times a day, he increased the dose to three quarts of unmixed carbonic acid air every hour. No sensible effect was produced upon the disease, sometimes the respiration of the facitious air was attended with difficulty. Dr. Hempel of Gottingen, relates a case in which the carbonic acid air was used for a few days in the very last stage of consumption, with no other effect but unpleasant sensations immediately after the first respirations; viz. faintness, anxiety, dyspnoea, quickness and inequality of the pulse. These feelings soon subsiding, a burning was felt in the left side, which was the supposed seat of the disease. By degrees, the lungs became accustomed to the air, and no effect followed

its use. The proportion of carbonic was then increased from one third to half ; and the same uneasy sensations were produced. Mr. Ehrhart, the botanist, towards the close of consumption, having taken carbonic acid gas much diluted, for seven days, left it off because it seemed to disagree. Dr. Buckner tried carbonic acid gas for four days in a case of phthisis extremely far gone, and reports that it occasioned pain in the chest with dyspnoea.

It has been since stated, that Dr. Girtanner's first patient after his journey of twenty german miles on foot, had a violent relapse ; and that he died on the 20th of Nov. 1795. " The strongest proof, says the account, of the great relief he experienced, was the firm faith he had in the air to the hour of his death. He bespoke an apparatus and was about to dress to go out to hasten the workman, when he sunk lifeless into the arms of his attendant. It should be told that he had in his childhood an ulcer of his lungs, in consequence of a blow, which was apparently healed by the *suc d' herbes* ; and that almost every spring

“ he suffered from cough, dyspnœa, and purulent expectoration, symptoms which commonly disappeared on the approach of summer.” Dr. Girtanner mentions further, as a fact to the present purpose, the case a person of forty, excessively thin, who had been plagued with an obstinate dry cough for four months, which would yield to no medicine, not even to opium. “ This man,” says he, “ assisted me in my experiments with fixed air ; and in its preparation was obliged daily to inhale a portion. In a fortnight his cough was quite gone and returned no more.”

Thus scanty is our foreign information. It seems surprising that a very obvious suggestion was never carried into practice. For as in a large proportion of the cases the carbonic acid, taken in small quantities and at long intervals gave relief, what was more natural than to try a continued application of the same power ? Could more benefit be expected from the small dose of a quart, than what Dr. Muchry tells us was repeatedly experienced ? viz. that patients within a fortnight of their death should express

in the strongest terms, their sense of the alleviation they experienced. Dr. Kortum, the author of the work (reputed classical) *de vitio scrophuloso*, and a busy practitioner, by no means sanguine in his expectations from new projects, has given some information, in my judgment, of greater value than the preceding direct trials of factitious gas; and has subjoined reflections that have my entire assent. "The following facts," says Dr. Kortum, tend in my opinion to confirm the use of mephitic gases in consumption. The mineral waters at Aachen are known to contain sulphurated hydrogen gas in larger quantity, and to deposit more sulphur than any water, hitherto described, in Europe. They also contain much carbonic acid. Both gases are continually exhaling from the numerous hot-wells and streams, so that the hepatic smell extends over the greatest part of the place, and is particularly strong near the wells. It is an old observation, that sulphureous vapours are wholesome in consumption;* and it is truly

* The contrary opinion, I think, is more general, at least in this country. Editor.

striking to observe how few consumptive patients are to be found in Aachen, while in the circumjacent country the complaint is very common. According to the most accurate information I have been able to obtain, consumption is here very uncommon ; and I have known consumptive persons much relieved by settling in Aachen. Does not this depend on the gases from the hot springs, particularly the hepatic ? Should my idea be confirmed, a most convenient apparatus might be established here for the respiration of this gas by phthical patients. Taking a few inspirations of a mephitic gas at stated hours can do nothing. The gas must be mixed with atmospheric air in such proportion that the patient can constantly remain in it. At Monjoye, six german miles from Aachen, a place that lies very high in the mountains, and where of course the air is very thin and piercing, consumption is so frequent, that according to the recent observations of a resident physician, *one half of the inhabitants are cut off by it*. Inflammatory pectoral complaints are much more common than in the neighbour-

ing low country. This confirms Beddoes's observation concerning the greater frequency of consumption, and of inflammatory disorders in elevated situations."

Whatever may be the dearth of adequate experiments, the proposal for trying gases and vapours has created abundance of discussion abroad. It has obtained repeated notice from the contributors to the most popular journals in Germany; viz. the *Gottingen Review*, *Hufeland's Journal*, the *Salzburg Medical Review*, and the *Gotha Journal for inventions, theories, and contradictions in physics and medicine*; from which publications I have borrowed most of the preceding intelligence. The most respectable and celebrated of the medical philosophers on the continent, have expressed sentiments, favourable to the general design. Writers, otherwise liberal and candid, have inadvertently assumed that I proposed the inhalation of gases as a remedy, confirmed by my own experience, whereas, I only urged their full trial. Nor from the published facts did I ever pretend to draw any but this general conclusion. *Since*

facilitious airs have been so freely used with so little injury, may we not safely persevere till their virtues or want of virtue shall be ascertained? And however strenuously I may have contended for the propriety of the investigation, I have been careful expressly to subjoin, that *I would not venture to give the smallest assurance of success in any one denomination of disease.* Besides those that approved and those that unwarily misrepresented, there was a third party; and this “not the better portion of the German physicians; which says Dr. Girtanner, reviled the author and his project in the coarsest terms, “just as if it were a crime to try to alleviate the “afflictions of humanity, or to propose new “and powerful means against incurable diseases.” The reader will not be surprized that I should have often been amused by critics of this description, when he is informed, that some of them even tell the public that they incline to believe “that I am *totally ignorant* “what oxygen gas is (*Journal der Erfindungen** St. 17. s. 68.

* The surmise arose from confounding what I say of the respiration of *diluted* and *undiluted* oxygen gas.

Opinions, equally honourable to themselves and to the subject of their strictures, have been expressed by some of those British literary ruffians who engage by the day, or the week, or the month, to assassinate literary reputations on account of delinquencies, not literary. This is in the order of motive and conduct. But one cannot help feeling concern when a pains-taking observer* gives vent to his spleen in such language as the following. Dr. Fordyce (*3d. diss. on fever. p.p. 173, 4, 5.* after asserting that

* Dr. Crichton (*Mental derangement, i. 46*) says there is scarce any treatment of consumption but has shewn "equal, if not superior" powers to a reduced atmosphere. The repose alone, which I have often known follow the use of gases, seems to shew that this is a mistake. I have nowhere said that occasional small respirations of gases and vapours have cured, or promise to cure, consumption. And where have they been kept constantly applied to diseased lungs? Can Dr. C.'s learning supply a satisfactory reference to facts of this nature? Dr. C. asserts (p. 35) that I have adopted Dr. Girtanner's opinion concerning irritability. This is false. In my earliest conjectures (*Obs. on calculus, p. 264*) I protested against this interpretation of my words: and Dr. C. should have attended to what I have since written, since he chose to notice my opinions.—As to Dr. C.'s book, I regret that he had not made himself master of our best writings on pneumatology. To quote from an unknown foreign work, gives in the eyes of some readers, an awful air of erudition. For myself, I always thought those German magazine stories about mad people, fitter for chimney corner gossiping than for philosophizing on human ideas and feelings. The use made of them by Dr. C. has nothing altered my opinion.

oxygen gas “ does not sensibly affect the mat-
 “ ter of the body, except that it adds yellow to
 “ the red particles of the blood, which is so
 “ altered in the circulation through the body,
 “ that it must pass through the lungs, and from
 “ them again into the other parts of the body,
 “ in order that a man should exist” introduces
 a folly better calculated to convulse a circle of
 pupils, raw from the pestle and mortar, than to
 satisfy persons who think. “ Whenever, says the
 “ Doctor, any new and seemingly important
 “ fact has been discovered,----- mankind in
 “ general, and very often even practitioners in
 “ medicine, conceive it must be applicable to
 “ some medicinal purpose. Just as an infant,
 “ allured by any thing which glitters in its eye,
 “ applies it to its mouth, supposing* it must
 “ be likewise excellent food; so infants in
 “ medicine are dazzled with any surprising
 “ discovery, and immediately employ it for
 “ the cure of diseases, not considering how
 “ extremely difficult an art medicine is;

* The Doctor is, I believe, mistaken in regard to the
 infant's motive.

“ how fallacious experiments made in it often
 “ are, as has been observed long ago by Hippo-
 “ crates, and by what slow degrees valuable
 “ medicines have had their powers investigated ;
 “ how long it was before the effects of the bark
 “ of cinchona, of mercury, of antimony were
 “ brought to light, as far as they are already
 “ known.” Oxygen gas however is not a thing
 foreign to the actions of life : and it appears
 much more peevish to throw out such insinua-
 tions, than childish to suppose it applicable to
 “ some medicinal purpose.” It is by Dr. F.’s
 confession, necessary to existence : Is it not
 likely then by varying its proportion, that we may
 vary the mode of existence ? We also *do* know
 much more of its effects on the matter of the
 body than its adding a yellow to the red of the
 blood. Mr. Lavoisier’s experiments on respira-
 tion and those which I have published, demon-
 strate its great power on the actions of the
 arterial system.—As to the rest, considering
 what an apprenticeship mankind have served to
 the art of making experiments, we may hope to
 be able to bring out certain results sooner than

could be done by the antients, or in those half-barbarous ages, when controversy ran high concerning antimony, mercury, and bark. And the longer we are likely to be before we arrive at the end proposed, the sooner ought we to start, and the most briskly to advance.

In the fourth part of *Considerations on airs*, p. 123. and fg. I have given reasons for supposing that living with cows would be serviceable in some consumptions. I have frequently since recommended the practice, but for the most part, as might be expected, in vain. However, in four cases the experiment was made. In the first, the patient's bed was brought to the mouth of a closet and enclosed with canvass, the cow being brought every evening into the closet. During the night the patient had the animal's breath in abundance; but none of those fumes from putrefaction, on which I apprehend the benefit to depend. It was a case of the last stage of consumption. No effect, good or bad, followed.—In a second case, the patient *lived* with three cows, without benefit. The treatment was not conducted under my

inspection ; and as the cows were brought into the dwelling-house, I am not informed whether the room was carefully cleansed of the cow-dung and urine. I supposed this to be a case of tubercular consumption. The third case was that of a person with black eyes and hair ; so far advanced, that I was doubtful whether my patient could support the journey from Bristol to his feat near Leeds, where he determined to make the trial. His brother, Mr. W. Greenwood of Leeds, gave me the following account of the result. “ The remarkable case of the lady, gave my brother and his friends hopes of the possibility (if not of a cure, at least) of such relief as might protract his time. He therefore proceeded according to the directions in the case alluded to, but *not* with the same success. I am sorry I cannot give you an accurate statement of the progress of the experiment, or of its effects on the patient. The air was of the most balmy fragrance, warm, yet refreshing, its smell not unlike that of new-mown hay. The thermometer in a morning (the door having been shut all night) was generally at 90° or

above. In the day, fresh air being frequently admitted, it stood between 70 and 80. It is to be lamented, he did not make this trial sooner, for *as it was*, there is no doubt but it not only lengthened his days, but *greatly alleviated* his sufferings. Had it been begun in an earlier stage of the disorder, it is not to be said how "favourable the event might have proved."—The result of the fourth case may be seen in the subjoined statements. Mr. Sandford says, "In compliance with your wishes and my own inclination, I have procured from Miss E. Seward, some account of the great benefit her late lamented brother, Dr. Seward, experienced by living in a cow-house for about six weeks previous to his death, and which he was induced to make trial of, in consequence of the well-authenticated letter on its good effects in pulmonic affection, published by you in the fourth part of "*considerations on factitious airs*," p. 123. And here it may be necessary to remark, that previous to the adoption of this plan, he inhaled various modified airs, though with very inferior success to that of living in a cow-house. I visited him

twice during the time he principally spent in it. The building was constructed with large *stones* and mortar, *such* as abounded in great plenty in the neighbourhood where he then resided, after quitting Worcester. It was built up to the gable end of a brick barn, which formed one of its walls, and a sloping thatched roof, supported by three stone walls, of sufficient dimensions to contain four cows, constituted the building. The height from the ground to the bed-room, was about eight feet, and the same from that floor (which was open every other plank) to the highest part of the sloping roof. He slept, and remained in the room about sixteen hours out of the twenty-four, when he walked not more than four or five yards to his mother's house, and which was directly opposite to the cow-house, to eat his dinner, and to enjoy in some degree, the society of his affectionate family. But latterly, (as his sister has faithfully related) he found himself so much more at ease when in the cow-house (the temperature of which, by a thermometer kept constantly there, was about 65 or 66°,) that he

could not be prevailed upon to quit it; and in which, he calmly resigned his valuable life on the 14th of December, 1796. To you who know and esteemed his worth, I need say no more, than lament he had not earlier adopted this plan, by which many of his most harraffing complaints were greatly alleviated."

"The late Doctor Seward slept in a cow-room about six weeks; he left it by day to enjoy the society of his friends; though he frequently said, that when in the cow-room his respiration was easy, and when he coughed it was with less difficulty, and not so frequent, as expectoration immediately followed the effort of coughing.

When in the cow-room, Dr. Seward seldom expressed by his manner, sensations of uneasy feelings, which towards the termination of his disease, continually shewed themselves when not in the cow-room, by the repeated desire of having the air changed, and frequent use of a fan. When he took to his bed, he said that he had each day repented leaving the cow-room, and that he should survive it a few days altogether; he lived but four days after this, and

was chearful and easy except when coughing, till the last day. His difficulty of swallowing was much abated after he staid in the cow-room, nor was it worse in the evening, as it had usually been; he took nourishment frequently, seemingly with much pleasure, but was then in a very weak state, and in a constant perspiration."

E. S.

Whether the effect experienced in these two last instances do not warrant us, in some less advanced cases, to expect a cure from the *continued* application of certain gases and vapours, even though it be ascertained that their occasional inhalation be of no avail, the editor leaves to the judgment of his readers.

Summary of the late Dr. GEACH'S practice in low fever.

By Mr. S. HAMMICK, Junr.

I do myself the honor of communicating to you the following sketch of treatment pursued by my late learned and truly invaluable friend, Dr. Geach, for several years past, in the typhus,

low, nervous, contagious, or putrid fever, (as it is generally called), with great success; an account of which treatment, had he fortunately lived, it was his intention to have published the ensuing summer. The reason of its being prevented we must all most seriously lament; for a treatise issuing from his pen, on the beneficial effects of calomel and antimony in this disease, would, no doubt, have been so amply stored with facts and observations, as to have roused the universal attention of medical men to the subject.

The Doctor used candidly to confess, that he was led to this practice at first, about thirty years since, whilst attending the crew of a large Russian ship, which had been driven into this port in the greatest distress. After encountering several gales of wind, her people from great fatigue and uncommon exertions, had become very sickly, and the typhus fever raged with great violence amongst them, accompanied with symptoms of great malignity. He then observed that the only men who escaped the contagion on board, were men

under the influence of mercury, which they had taken for the cure of the lues ven. This fact made great impresson on him, and ever since that time he had been accustomed to give mercury in such fevers, but not with such freedom till the last seven years of his practice, and for the last five years whilst I had the honor of being an assistant surgeon placed under him in this hospital. I have seen him prescribe it, and have prescribed it myself under his own immediate eye and controul, whenever any person was seized with that fever in the surgical wards of this hospital; and as I always attended him during that time in his visits of the wards, the number of cases has been considerable: and I have also seen it very successfully administered in some very alarming cases of typhus gravior among the poorer class of inhabitants of Plymouth Dock, and Stonehouse, whom humanity induced him to visit in those places, and to whom he had the goodness to take me, in order to be thoroughly convinced of the efficacy of this remedy, and thereby induced, from actual observation, to give this

medicine with confidence in my future practice; for the doctor thought this plan only wanted publicity to obtain a preference to those usually employed in such diseases.

The following is only an imperfect outline of the plan, but even as such, I trust it may not be deemed unworthy of your perusal—imperfect, as I have not had access to his notes and observations, but at the same time I pledge myself for its faithfulness.

Whenever the doctor was called to a person labouring under symptoms of typhus fever (if within two or three days of its first attack) he used constantly to prescribe fourteen or sixteen grains of ipecacuanha, assisting its operation with chamomile tea; three hours after the cessation of the vomiting (if the patient was delicate), a bolus of five grains of calomel, with a scruple of rhubarb was given, but if the patient was of a strong habit, a scruple of jalap, with eight or ten grains of calomel, were administered. If evacuations were not thus produced within eight or ten hours, castor oil, or some other laxative, were given occasionally till the desired effect had taken place. The windows of the

room were opened in such a manner, that the room was kept perfectly cold, without subjecting the patient to a current of air ; the bed-curtains nearly all withdrawn, so that free circulation was admitted, even in winter ; taking care to have (where it could be procured), frequent changes of linen. After the stools, the following boluses were immediately ordered :—calomel ~~eight~~ ^{three} grains, pulv. antimonial. four grains, conf. cynosb. q. f. ut f. bol. to be taken every six hours when the symptoms were slight, but when the case was very urgent, or he had not been called in till the fever had made some progress, then the above quantity was given *every four, three, or even every two hours*, permitting weak lemonade, tamarind, or cream of tartar water to be taken for the common drink. If the fever still went on, and the patient's strength became exhausted, a little port wine diluted with water was allowed ; usual quantity half a pint, seldom or never exceeding one pint in twenty-four hours. To any person unaccustomed to give these boluses, diarrhœa, ptyalism, or vomiting, would naturally suggest themselves as the inevitable consequences in

almost every case of their exhibition ; but the fact, in a multiplicity of instances, directly proves the reverse ; for in general we are obliged to order a little castor oil, rhubarb, with kali ppt. or an electuary, made of equal parts of cream of tartar and conserv. cynosb. Ptyalism has seldom, as I have before said, followed their use, notwithstanding they have been continued to some patients every *three* hours, for eighteen or twenty days : but when they did affect the salivary glands, the cure was always certain and expeditious after that event, appearing to check immediately the progress of the disorder. When diarrhœa supervened, the doctor was cautious how he checked that discharge, never attempting it, unless the patient was very feeble or low ; for in several instances where numerous stools have been procured, the patients have found themselves relieved of a delirium which had been on them for three or four days before, but when the diarrhœa continued profuse, exhausting the patients strength, then means were employed for its removal, commonly a scruple of conf. opiat. or an ounce of poppy syrup sufficed : if they did not, half a grain

or a grain of opium was combined with the calomel and antimony, but seldom were we necessitated to seek the assistance of opium, and in no other way did the Doctor ever administer opium in this disease. Vomiting when excited, was commonly allayed by the saline mixture in the state of effervescence ; when this symptom much harraffed the patient, the antimonial powder was reduced from four to two grains : this was the system pursued throughout the whole of the stages of this fever, never administering any other medicine, unless any extraordinary occurrence took place ; therefore the whole dependance for a cure may be clearly perceived to be entrusted to the calomel and antimony. In some few cases, when delirium was great and the head much affected, a blister was applied to the nape of the neck ; as soon as signs of amendment appeared, the boluses were discontinued, *and not till then* ; a little mutton broth or jelleys were allowed and a decoction of bark with bals. tolu was given ; but the bark in substance was never given by the doctor ; for the bad effects of it in this form, when exhibited to weak stomachs, far outweighed, in his

opinion, any good it ever produced. It is well worthy of remark, that in all those cases where the symptoms were very urgent, and the putrid appearances more apparent, that there the boulders scarcely ever were observed either to ruffle the bowels or stomach.

Now, Sir, after the above statement, it may be expected that we have some theory to defend this innovation of practice, and that I ought, after troubling you in this manner, to attempt accounting for the *modus operandi* of this medicine, which, in a variety of instances, I freely confess myself incompetent to do, and even were I capable, unwilling to do ; for in theory we may be overthrown in various ways, but in the above account we never can ; for who can overthrow us when truth is our foundation ? Feel assured, sir, this was the mode of practice pursued by Dr. Geach in those cases, not only in this hospital as first Surgeon, but also in a most extensive range of private practice, and of course nothing but the success attending it, could have induced him to persevere in a treatment so widely different from that pursued by other practitioners. Should you be disposed to

propose any questions to me more explicit of it, I shall most readily answer them, and consider myself at the same time, honored by the application. Or should you deem this letter worthy of being shewn to any of your medical friends, the freest permission accompanies it.—I am, &c.

STEPHEN HAMMICK, Junr.

To Dr. BEDDOES.

P. S. I beg leave to state, that my father, about five years since (in the absence of the physician) at the recommendation of Dr. Geach, pursued the above plan, with very great success, in a number of cases of typhus gravior, received into this hospital from his Majesty's ship Squirrel, on board of which ship, the fever had been so violent, that the Board of Admiralty gave an order for destroying the bedding and cloaths of the men, supplying them anew at Government's expence; and also, that my friend, Mr. John Fryer, Visiting Assistant Dispenser at this Hospital, who, when a fever of the worst species of typhus was raging among the french prisoners

confined at Mill-Prison, Plymouth, about three years since, and at a time when most of the Assistants there employed, were confined by the fever, nobly and humanely volunteered his services, found that the calomel and antimony was very far superior to every other practice. Of their testimonies, it was the Dr's. intention to have availed himself in the purposed pamphlet.

Royal Hospital, Plymouth, Oct. 21, 1798.

To Dr. BEDDOES.

Since the preceding communication came to hand, the Editor has received from Mr. H. a note to the following purport. " Since I did myself the honor of transmitting you the outlines of Dr. Geach's treatment of typhus, I have found that the Dr.'s unfinished manuscript on that practice, hath fallen into the hands of a professional and intimate friend of mine, Mr. Knighton, of Plymouth-Dock, one of the late Dr.'s bosom friends ; which I have pressed him to publish, and have the satisfaction of saying,

he intends it, together with some of his own practical observations. The above notice of Mr. Knighton's design, I should wish you to make known to the public, by inserting it in the publication in which my sketch of that practice will appear."

Extracts of Letters, from Mr. COOKE, Apothecary, Gloucester.

As I have had frequent opportunities of making enquiries upon the very important subject of Dr. Jenner's ingenious treatise, during an extensive inoculation in various parts of this country (within the last two months) I wish to communicate to you a recent case (well authenticated) which I think, will sufficiently prove, that the cow-pox is not an infallible or invariable preventative against acquiring the small pox ;

" unless the cow-pox is only a positive preventative
" against acquiring the small pox in a natural way,
" or unless natural changes in constitution from
" certain periods of life, may have any influence in
" diversifying the propensity to particular diseases.

December 5, 1798, I inoculated Mrs. Carter, of Longney, in this County, in the left arm, with variolous matter taken from a patient having the distinct small pox. The arm did not appear much inflamed for several days, but upon the seventh, suppuration took place. She sickened upon the ninth; the eruption commenced upon the twelfth; she had rather a burthen of pustules, is now recovering without any variation from the common course of inoculated small pox.

When Mrs. Carter applied to me, respecting the small pox, it was to inoculate her son, with some other patients in the same place, but she said, "as she was formerly most violently affected by the cow-pox, and had frequently been with persons who were infected with the small pox, both in the natural way and from inoculation, without catching it, she thought it unnecessary to be inoculated herself, particularly as she had read in the newspaper that persons once affected with the cow-pox, could never have the small pox" I remonstrated and advised that she should be inoculated,

observing, "that provided she did not have the disease, no harm could possibly ensue from "being inoculated;" she therefore consented; and the sequel proved that she either never had the proper cow-pox of Dr. Jenner, or that the cow-pox is not a preventative (invariably) against the small pox. Mrs. Carter is fifty years of age, had ceased to menstruate for several years, is a very healthy woman, and gave the following account of her having the cow-pox.—When eighteen years of age, she lived in a dairy farm, in Longney; at that time the cows were affected with chopped and sore teats, all the servants who stripped these cows, had inflammation and boils upon their hands. She was so ill with fever, and these boils, that she could not work for a week; her hands and arms were dreadfully swelled, and she kept her bed for two days. In this state, she applied to Mr. Cork, who then was in practice at Frampton, in this County, he told her, "she had the cow-pox very bad, and that it was a disease the "nearest to the small pox that could be." Her fellow-servants all recovered without medical

affistance, by the use of a frequent application of bole armenia, but it did not succeed with her, for Mr. Cork, upon seeing her, made incision in various places about her arms and hands, and she has evidently the marks remaining at this time. Mrs. Carter has lived in Longney ever since, and many of the neighbours recollect her being ill with the cow-pox.

I have only to add, that as I pass this woman's door every other day, if I have omitted any enquiries, I will make what others you may think necessary, since it will give me infinite satisfaction to ferret out (within the boundaries of my practice) every circumstance that can tend to establish or confute Dr. Jenner's opinion "that cow-pox most compleatly destroys
" the propensity to, (or the power of infecting
" the human body with) the small pox."-----

In order to satisfy my own mind, that the case of Mrs. Carter could admit of no doubt, I have used every endeavour to obtain the best information in this neighbourhood, upon that disease, termed the cow-pox, and upon its effects when communicated to the human sub-

ject. I now beg to submit to your perusal, the result of my enquiries, and I think it will sufficiently prove, that we ought to desist from every attempt to eradicate the small pox, by inoculating with cow-pox matter, since the utmost to be expected, is the procrastination of a disease more dreaded than any other to which we are liable.—Besides, we are told, that the introduction of this animal disease into the human system, although never fatal and shorter in its progress, is more violent in its operation, than the introduction of the small pox. Why then prefer this disease of brutes, to one which we are well assured is peculiar to man (and by no means fatal, when the patient is not unhealthy or under dentition)? The former is probably only a local disease in the brute, the latter, we know to be a constitutional disease in man. An indefinite period of time can only determine the effects of the former, (and it seems probable, that whatever its influence respecting the small pox may be, it is in a certain time entirely destroyed) whereas, the latter disease may be intended for certain useful purposes in

the human œconomy. If the introduction of diseases peculiar to the brute creation, or animal poisons could either prevent or eradicate the virulence of human diseases, which are as yet ungovernable by any known remedies, there could not be a greater acquisition to medical knowledge. But since it will appear, that cow-pox cannot have any specific effect in preventing the small pox, the public mind ought to be at once relieved from its present suspense, by every satisfactory evidence that can be brought forward upon this subject.—I trust I shall be acquitted of any other reason for communicating the following facts, than my respect for that profession, of which I am proud to be a member, and from considering it my duty, to report what I have heard or seen upon a subject, which at present so much agitates the public mind.—I am, &c.

CHARLES COOKE.

Mr. Clayton, living in Gloucester, says, that he is in the habit of attending the cows, at the dairy farms, in most parts of this County, for

ten miles round Gloucester : that the constitutional diseases of cows are by no means numerous (many of which are anomalous and rarely to be met with,) but the most frequent to be observed, are the following :—The red water, the blain, the yellows, and the murrain ; these are all the diseases that can with propriety be nominated constitutional, because the fevers to which cows are liable, are generally symptomatic, and arising from local pain : that the local diseases are very few, of which, the lough, swellings of the udder, and cow-pox, are nearly the whole to which a name can be given ; the two former are most common, the latter rarely to be seen, excepting in the spring and summer seasons : that the cow-pox begins with white specks upon the cow's teats, which in process of time, ulcerate, and if not stopped, extend over the whole surface of the teats, giving the cow excruciating pain :—that if this disease is suffered to continue for some time, it degenerates into ulcers, exuding a malignant and highly corrosive matter ; but this generally arises from neglect in the incipient state of the disease, or

some other cause he cannot explain :—that this disease has not a regular process of commencing and terminating without a remedy, because, if not attended to, it would end in a mortification of the teats, and probably death of the animal :—that this disease may arise from any cause irritating or excoriating the teats, but that the teats are often chapped without the cow-pox succeeding. In chaps of the teats, they generally swell ; in the cow-pox the teats seldom swell at all, but are gradually destroyed by ulceration :—that this disease first breaks out upon one cow, and is communicated by the milkers to the whole herd, but if one person was confined to strip the cow having this disease, it would go no farther :—that the cow-pox is a local disease, *and is invariably cured* by local remedies :—that he never knew this disease extend itself in the highest degree to the udder, unless mortification had ensued, and that he can at all times cure the cow-pox in eight or nine days, by his usual local remedies :—that he is conversant with the diseases of the horse, and extensively employed, particularly in curing the grease :—

that he cannot recollect ever to have had horses with the greafe and cows with the cow-pox, under cure at the same time, and at the same farm:—that he is very certain, he has frequently had cows with the cow-pox, where no horses whatever have been kept:—that he considers the greafe as a name, having great latitude in the diseases of horses, because, sometimes the cure of it may be effected merely by topical remedies, and at other times, it is only to be compleated by internal remedies:—that he does not consider the greafe an infectious disease among horses, since greasy horses and horses in perfect health, frequently stand in stables together indiscriminately, without infecting each other; and although it is probable if the discharge from greafe was to be applied in its most acrid state to the heels of a sound horse, it would inflame and excoriate them, yet it would not produce the greafe:—that the greafe is most prevalent in the winter, at which time, he has never known the cow-pox to occur, and therefore, cannot think it at all probable, that the greafe can have the least influence in

producing the cow-pox. There is little variation from this account, in the information I have obtained from some of the most respectable dairy farmers in this neighbourhood. Those who have seen the cow-pox among their domestics, all agree, that if they have been soon afterwards inoculated for the small pox, they have had the disease very slightly, but since the late general inoculation, are as fully satisfied that many have had the small pox in a more decided manner, who some years before, had the cow-pox very severely.

Conversing with Mr. —, an eminent and very extensive practitioner in this County, upon the cow-pox, he said, that he was confident every person who previously had had the cow-pox, as certainly had the small pox afterwards if they were inoculated, but in a much slighter degree, and where any eruption succeeded, it was without maturation:—that the arm of a person previously having the cow-pox, assumed a singular appearance while under the action of small pox from inoculation:—that the fluid in the arm inoculated, arising from the previous

inflammation, was merely lymph, as the incision never suppurates :*—that he has frequently inoculated other patients with this lymph, with equal success, as if he had used the natural variolous matter :—that he never recollects to have seen a patient having the cow-pox, but after having inoculated one or two persons, who said they had previously had the cow-pox, he could always tell, whether any of his patients under inoculation, had had that disease, from the progress and singular appearances of the arm :—that during a practice of forty years, the number of those having previously the cow-pox, does not, he thinks, amount to more than sixty, although he has inoculated many thousands for the small pox :—that he has, by way of experiment, a second and third time inoculated persons, who having first had the cow-pox, had been afterward successfully inoculated for the small pox ; and no further effect was produced

* I will take the first opportunity of ascertaining, whether the same appearances and progress of inflammation would occur, provided a patient, who before had the cow-pox, was inoculated in the thigh, for the small-pox.

than upon the arm of other persons who before had the small pox, either from inoculation or in the natural way. Mr. Beach of Hardwick, informed me, since writing the above, that Farmer Tombs, of Hasfield, near Gloucester, who had the cow-pox many years ago, caught the small pox in coming to Gloucester market, and died of it, in December last. He says, that several of his neighbours can testify Mr. Tombs had the cow-pox many years ago, but was rather cautious in going to any place where he knew the small pox particularly prevalent, although he never would consent to be inoculated, thinking he was safe :—that lately, Mr. Tombs, was not at all afraid even to enter an house where he knew the small pox to be, and consequently at length, fell a victim to that disease.

CHARLES COOKE.

*Letter from Mr. THORNTON, Surgeon, Stroud,
dated Feb. 7, 1799.*

In consequence of your letter to me of the 1st. inst. I send you the following account of my experiments relating to the cow-pox. On

the 1st. of December, 1798, being informed that the cows on Stonehouse-Farm had the cow-pox, and that a man who milked them was infected with the disease, I called on him that day, and found him with pustules on his hands and fingers, which had made their appearance four days before. The patient had not had the small-pox: the symptoms he experienced previous to the eruption (he told me) were pain in his head and in the axillæ, with frequent cold shiverings, fever and debility; on the second day the cow-pox broke out, which terminated his complaints. I immediately procured some matter from a purulent pock, which was the only one that was not degenerated into a sordid and painful ulcer. I, that evening, went to Stafford's-Mill, and inoculated Mr. Stanton and four of his children, the eldest was ten years old, the youngest about ten months. On the third day, all their arms appeared to be under the influence of a very active virus; the arm of the youngest child was affected with a kind of erysipelatous inflammation, the size of a half-crown piece, without any elevation of

the cuticle, it was half an inch above the place where the matter was inserted, with which it did not seem to be in the least connected ; on the fourth day, the inflammatory appearances of the three eldest were increased ; the youngest child's arm had lost that efflorescence, but about the puncture the redness was increased : Mr. Stanton's was evidently on the decline, and from this time gradually died away. On the sixth day, the skin round the incisions of the children's arms was considerably elevated, and contained a limpid fluid. The inflammation in each kept on till the fourteenth day, when the punctures began to be covered with a crust of considerable thickness, from which an ichorous matter continued to discharge for several days, without any diminution of the surrounding inflammation. About the twentieth, the scabs fell off, and the inflammatory appearances subsided. During the whole process, there was no commotion excited in the system, nor the least pain or uneasiness perceived in the axilla of either.

From the long continued local excitement,

I began to entertain a hope that the virus might imperceptibly have crept into the habit, and proved a security against the variolous infection. To relieve my own doubts, and to ensure the safety of my patients, I had immediate recourse to the introduction of the small-pox matter. All the children received the infection, and passed through the different stages of the disease in the usual slight manner. Mr. Stanton's constitution resisted my repeated attempts to communicate it to him. I therefore conclude, as he spent the early part of his life in London, that he might have had the small-pox slightly during that period.

Concerning Mr. Colborne's children, I have received authentic information " that three of
 " them were inoculated with cow-pox matter,
 " together with a servant-man ; two of the chil-
 " dren suffered severely from violent inflam-
 " mation and alarming ulcerations in their
 " arms. They were all inoculated afterwards
 " with the small-pox matter ; the two whose
 " arms had been so dreadfully affected, did not
 " take the small-pox, the others received it."

A a

If you should think this communication deserving a place in your intended publication, you may insert it.

EDWARD THORNTON.

Dr. BEDDOES.

P. S: Some cases of cow-pox have lately occurred in this neighbourhood ; if on further investigation they appear to throw any light on the subject, which they bid fair to do, I will take the liberty to let you know the result of my inquiries ; they at present appear to operate against Dr Jenner's doctrine of security:

*Answers to Mr. (now Dr.) Adams's Queries,
concerning the Srvens.*

1st. If the disease ever appears without producing a local effect on the part where it may be supposed to be received ?

The disease almost always begins with an inflammation on the velum pendulum palati, and uvula, and afterwards on the tonsils, of a dark red colour, which is succeeded in one or two days, or sometimes so late as six or eight, by small pimples or vesicles ; which leave ulcers, with a white lardaceous surface, and red abrupt edges. These spread, and after some time, dark red spots which fall out into ulcers, appear about the perinæum and anus. Very frequently fungous excrescences, arise round the anus, instead of the red spots ; these gradually increase and ulcerate.—I have been assured by several patients, that their sore throat had been very slight, and soon went off, without using any remedy, but was succeeded by the appearances just mentioned. In these cases, I am inclined to believe, that only a slight local inflammation was excited by the virus as it en-

tered the system; in the same manner as we sometimes meet with a bubo, as the first symptom of lues, without any preceding chancre. It is perfectly ascertained, that the breath of people labouring under the sore throat, is loaded with infection, and communicates the disease, without the contact of ulcers.

2d. If there is any, and what difference, between the primary and constitutional ulcers?

The primary ulcers, in the throat, inside of the cheeks, or lips, (which last are frequent in infants, probably from the infected nurse kissing them) have always a white surface, with red thickened edges. The secondary ulcers, on the perinæum, belly, thighs, &c. have the same appearance at first, but after having continued long, they are often of a bright florid red, like cancerous sores; the discharge is thin and acrid, and they are then much more difficult to cure. This change in old ulcers from fivers, I am inclined to think, is the natural progress of the disease, and not merely the effect of stimulating applications.

3d. Whether mercury cures the disease

without producing those effects on the system, which we find necessary in the cure of inveterate chancres ?

I am convinced from abundant experience, that a less quantity of mercury will cure this disease, than is necessary in the common lues venerea. The sublimate is the preparation that cures it most speedily ; and even in a few days after patients begin the use of it, the ulcers put on an healing appearance. It is necessary to continue the use of mercury for at least a fortnight or three weeks, after all the symptoms are removed, otherwise the disease will infallibly appear again in a few weeks or months. This holds true, whatever preparation is employed, whether sublimate, unction, or any other form. Salivation is never necessary ; patients may generally be allowed to go about.

4th. If the primary ulcers are cured before the constitutional ulcers have appeared, whether the latter ever shew themselves without the recurrence of the original ulcers ?

Nothing is more frequent, when the mercury is discontinued too soon after the healing of the primary ulcer.

5th. Whether in cases of phagedena, the parts usually heal by granulation, or skin over without the lost substance being renewed ?

There is certainly some small part of the cavity of each ulcer filled up by granulation ; but much more of the cure depends on the wasting of the surrounding parts.

6th. Whether the disease is ever cured by the uninterrupted efforts of the constitution ?

I am convinced it never is.

7th. If this be the case with phagedenic ulcers, whether if the funguses can be destroyed by applying escharotics as low as the sound part, the disease will cease in that part, and healthy granulations rise, no other remedy being used at the same time ?

I believe it is possible sometimes to remove an ulcer or fungus in this manner ; but the attempt will fail ten times for once that it succeeds ; and even when it has succeeded, the disease will break out again in the same, or some other part, in a few weeks.

8th. Whether mercury ever fails to effect a cure ?

I believe it never does, except in those deplorable cases, where from the long continuance of the disease, hectic symptoms have come on, and the constitution is so broken down, as not to be able to bear the remedy; precisely as happens in the common lues venerea.

The fivens certainly affects the mere surface of the body, more than the common lues. Buboës almost never occur in fivens. The bones of the nose and jaws are often destroyed by it, but I never knew any of the other bones affected; the patient dies hectic, from the very extensive ulcerations, before this could take place.

JAMES PATERSON.

Air, July 28, 1798.

Extract of a Letter from Dr. Paterson.

“ It is about fifteen years since I read Dr. Gilchrist’s account of the yaws,* in the Edinburgh Physical and Literary Essays, and I have not the book at hand; but if I do not forget, he considers the yaws, not as a particular spe-

* A country name for fivens. ED.

cies, or modification, of lues venerea, but as exactly and identically that disease. This I believe to be an error, and I beg leave to state my reasons for thinking so.

1st. Lues venerea was common in this country long before the yaws appeared. It is perfectly ascertained, that the latter disease was introduced into Airshire about the year 1745, by people who came from Dumfries to buy cattle. It was brought to Dumfries by a party of soldiers who had been stationed in the north Highlands; and the tradition there is, that it was imported or produced by the soldiers of Oliver Cromwell, labouring under lues venerea. Ever since its first appearance, it has prevailed in a greater or less degree, in the different places, at different times; sometimes abating so much, both in virulence and in frequency of occurrence, as to give hopes that it would entirely wear out; then breaking out again with greater violence, generally in the harvest season, and spreading over several parishes. I have often attempted to trace the infection to some person affected with lues venerea, but never with success. I

am convinced therefore, that it always arises from its own specific poison; whatever way that may have been originally generated. Some medical people in this country, imagine it a combination of the itch and lues venerea; while others believe it produced from the angina maligna and lues. With regard to its first production I know nothing; but I believe it is, like the small-pox and other similar diseases, never produced now *de novo*. The origin of these poisons is a very obscure subject, not likely ever to be much elucidated. *Latet causa, vis est notissima.*

2d. The yaws is undoubtedly much more infectious than the common lues; for it seldom gets into a family without infecting every person in it, and frequently spreads rapidly over a village. If the common lues were to spread in a similar manner, its progress in all large towns, would be truly dreadful.

3d. The yaws is certainly a more cutaneous affection than the common lues; for it almost never affects the large hard bones, and very seldom indeed occasions buboes. I admit, that

this may be partly explained from the different manner in which the virus is received, but I think not altogether. I look upon it as an ascertained fact, that, of all the preparations of mercury, the corrosive sublimate is the best adapted for the cure of fibbens; that is, it cures it more speedily, and with equal certainty with any other mercurial preparation. This probably arises from the fibbens affecting the surface of the body, more than the common lues.

4th. The yaws is more easily cured than the ordinary form of lues; for a much less quantity of mercury removes blotches and extensive ulcers, than is required in lues contracted in the ordinary manner. I have had abundant proof of this; but I acknowledge that the disease is very apt to return, from the patient's leaving off the remedy too soon. It is always necessary to continue it for some weeks after the symptoms have disappeared. Sauvages mentions a species of syphilis, which he calls syphilis indica, his third species: he says it is very infectious, but easier to cure than the other

species ; it appears similar to the yaws of this country.

These, Sir, are the only observations which occur to me at present, as worth communicating to you, on this disease. I have written them chiefly with a view to shew my inclination to aid you in your useful investigations." *J. P.*

On reference to Gilchrist, I find him asserting, that the fivens was " soon discovered to be of the venereal kind," that " if not the same, it has an exact resemblance both in its symptoms and cure", and that, " the solution of, corrosive sublimate has often been tried, allowing the patient to go abroad, but not with the desired effect." (Edinburgh *Essays*, III. 1771.)—I have no new information on the *siphyllis indica* of Sauvages ; but I will notice an academical publication, which came out four years ago, in hopes that some of our literary countrymen in Asia, may be induced to examine into the validity of the author's assertions. It is J. G. Klein, *Tranquebarra—Dani spec. inaug. de morbi venerei curatione in India orientali Hafniae*. 1795. Mr.

Klein contends that the venereal disease was known in the East Indies long before the discovery of the West Indies. The reasons assigned by the author are these: The antient medical writers, Sangarasiar and Aleffianambi, speak of this disease, and its cure by quicksilver, nine hundred and seventy years ago. Other writers still more antient, Tanmandari, Achastyer, Tirumuler Poger, speak of the history, symptoms, and mercurial treatment of lues venerea. The name *Moecha Wiadi*, is a Tamul word, and occurs in those antient authors. It is therefore unquestionably one thousand years since those works were composed.—It has been rendered probable by Hensler, Gruner, and Sprengel, in opposition to Astruc and Girtanner, that the venereal disease was not brought from America to Europe. But as in the case of the writers before the return of Columbus, so with respect to the Oriental writers above-named; the doubt is, whether their description is perfectly satisfactory. This doubt the members of the literary and philosophical society at Calcutta, may be able to solve. EDITOR.

*Case by Mr. G. Vise, Stilton, Hunts. dated
Feb. 1, 1799.*

ANN HIGBY, of Yaxley, in the County of Hunts. aged 27 years, was soon after the delivery of her first child, attacked with a quartan ague; it continued nine years, although every medical assistance was used. During all this time, she frequently complained of a pain on the left side. About the expiration of this time, the ague left her for twelve months: it returned from taking cold, and continued for ten years. On the intermitting days her health and spirits were very good during the whole of the time.—About the age of 48, her catamenia left her, and from that time she lost her ague. The pain and enlargement of the side began to increase, and from that time her body began to fill. In Feb. 1788, she applied to me for the first time: after giving me the above history, I examined her body, and made no hesitation in declaring it contained a fluid. Upon tracing the course of the side, I evidently discovered enlargement of the spleen. I told her I would

try what medicine could do for her, which if it did not succeed, I strongly recommended paracentesis. I used the digitelis infusion, in the manner ordered by *Whitbering*, without the desired effect. On the March following, I performed paracentesis, and drew off two gallons and five pints of dark-coloured water, nearly the color of coffee grounds. From that time nothing very particular occurred. I performed the operation ten times, from March 1788, to April 15, 1796. About the end of October, 1796, she applied to me to perform the operation again, but from some cause which I do not recollect, it was put off for a few days. Nov. 5, a few days after her application to me, I was sent for, the messenger informed me she had fallen down forwards on a brick floor, and had never ceased vomiting since the accident. I did not see her until the next morning, Nov. 6, when I was informed she had carried her bread to the baker's, in coming out of whose house her feet slipped, and she fell flat upon her belly; she was immediately deprived of the use of her legs, and was brought home in

a chair. Vomiting immediately followed. Upon enquiring, I found she had not made water, or had a stool, since the accident, every thing was rejected by the stomach. I immediately ordered her a common enema, with an ounce of salts, and the following medicine :—Conf. rosar. unc: unam ; elix. vitriol. acid drachm. un ; tinct. opii gtt trīginta ; aq. cinnam. unc: unam ; aq. menthæ q. s. f. f. mist. unciar : sex capt coc. h. duo seta quaque hora et repetat urgente vomitu. At the same time, I ordered the body to be bathed with warm water, afterwards rubbing it all over with the volatile liniment. On the 7th, enema repeated, and liniment continued, with the fomentation. On the 8th, enema repeated, mist : falinos : cum magnesia : alb: continuing the fomentation and liniment as before. On the 9th the same, 10th the same. The enemata were all rejected without fæces, and the vomiting continued very violent indeed. I frequently examined her body, and found it diminished in size every day, and I think during the six days, she vomited upwards of two gallons of fluid, nearly the colour of what

I used to draw off from her in paracentesis. 11th. I ordered the following pills : pil. ex coloc. cum aloe drach : un. pilul : xii. She took the whole of these pills before a stool was procured, during all which time, she never made water. After the operation of these pills, she made water, and recovered her strength very fast, and informs me she never had her health so well these thirty years. I called upon her since your last letter of Jan. 27th 1799, and found her in good spirits, but informed me she has had some little return of her ague within this last month, but has never had the least symptoms of dropfical complaints since Nov. 5, 1796, and has been perfectly well.

JOSEPH VISE.

TO DR. BEDDOES.

P.S. I have had one venereal case of my own ; so has one of my friends ; in both of which the nitrous acid did wonders—both were secondary.

On the use of NITROUS ACID in restraining sickness, by the EDITOR.

I have scarcely found any medicine so certain in its effect, as nitrous acid in relieving various sicknesses. Its power as compared with that of other acids, I cannot assign. The following instances, of which none but 4 & 5 are single in their kind, will suffice for the information of readers who may choose to put my assertion to the proof.—1. A lady of weak constitution, light hair and eyes, not chlorotic, liable to frequent inappetence and sickness at rising, was directed to take from six to ten drops of nitrous acid in water with sugar. On a great variety of occasions it never failed to remove the sickness. A repetition of the dose was sometimes necessary. 2. A boy with dark hair and eyes, subject to frequent sickness and bilious vomiting from various causes, as fatigue, a small excess in eating, taking cold—whose digestive organs

in short, seem to sympathize with every part of the system in disorder—has always been certainly relieved by a few drops of nitrous acid. 3. A person attacked in autumn with bilious vomiting and purging, had the symptoms immediately mitigated by nitrous acid, diluted and sweetened. Repetition of the acid soon carried off the attack. The same plan has equally succeeded in other cases of cholera—4: A gentleman, after eating freely of cheese much decayed, was seized with sickness and bilious vomiting. A dose of nitrous acid instantly relieved the sickness, and a second carried it off.—5. A lady in whose right hypochondrium a moveable hard tumour can be felt, complained of loss of appetite, weakness and emaciation, with pain in the stomach so violent, as to oblige her to take one hundred drops of laudanum, once or twice a week. The daily use of nitrous acid from twenty to forty drops, diluted and sweetened, much increased the appetite at first, and has kept the stomach for some weeks free from pain.—6. A gentleman, who had lived freely, and spent some time in a tropical

country, complained of obtuse pain in the right hypochondrium, and occasionally of more acute pain in the right shoulder. His countenance was fallow, he coughed hard, with expectoration of a little frothy mucus, such as are vulgarly called *sixpences*—pulse 120—fæces not sufficiently coloured—countenance fallow. Supposing the liver to be diseased, and the cough to arise from this cause, I put him on a course of nitrous acid, from a dram to a dram and half a day, occasionally discontinuing it for a few days, and directing oxyd of iron instead. In seven weeks he had no more cough; the pains were removed; the countenance was clearer; the fæces natural, and the pulse between 70 and 80.—Query. Would nitrous acid be useful and safe in the qualms of pregnancy? Would it remove the violent effect of digitalis on the stomach and liver? What would be the effect of a solution of oxygenated muriate of potash in any of the above instances?

Case by DR. LUKÉ, Physician at Falmouth.

A gentleman in the naval line, between forty and fifty, had by the immoderate use of spirituous liquors, reduced himself to the last stage of disease consequent to that unhappy practice. He had been compleatly jaundiced for several months, and at length, became so suddenly and generally dropical, that he was after the first week, so unwieldly, as to be incapable of stirring from his bed.—Several remedies had been used by the Surgeon of his ship, and for the first few days after I saw him, the most active diuretics were employed, without effect, until an operation became necessary, to relieve him from the intolerable distention. The evacuation of five gallons of water, by means of the trochar from the abdomen, afforded some ease, but the extremities were still enormously large, the pulse 120 and weak ; urine dark coloured, and not exceeding twelve ounces in twenty-four hours. In this state, he

was removed from his ship to lodgings, from an expectation that he could live but a few days

Directions were now resumed, and such medicines, as the most urgent symptoms demanded, but he was in two days as much distended as before the operation, so that it was proposed, to repeat it the following day. The necessity of it however, was superseded by the bursting of the orifice of the first wound during the night, by which several gallons of water were soon discharged.

Repletion and evacuation occurred in this way, twice more in the space of eight days, till at last, the operation of diuretics began to be apparent, and a sufficient quantity was passed by urine, to prevent the usual accumulation. He was now taking foxglove, and derived so much advantage from it, that his existence was more tolerable, but the quantity of urine was not more than equal to the daily effusion. Squills and calomel alkalis, æther, (Tickel's and common) were tried in their turn, during a month, without better success.

For the last fortnight, the stomach and bowels had been much affected. A dark coffee-coloured fluid was discharged, both by stool and vomit, and these symptoms increased daily to such a degree, that he could retain nothing in his stomach, and was often violently purged and griped. Effervescent draughts, tinctura opii-pills, with calomel and rhubarb, and many other medicines were given, without effect. It was at this moment, through a forlorn hope, I determined to give mercurials in such doses as might, aided by friction, affect his mouth in a short time. The accomplishment of my purpose, was more speedy than I expected. His mouth was sore on the third day, and twelve hours had not elapsed, from the time of his first beginning to spit, before the vomiting and purgings were mitigated, the stomach was sensibly relieved, and to my great surprise and satisfaction, in the space of a few days, retained food much better, than it had ever done since his illness.

Mercurials were continued, so as to keep up a gentle spitting for about three weeks, to which

diuretics were joined as before.—But although the good effects above stated, immediately succeeded, no alteration had been effected in the size of the abdomen and extremities.—His strength was nearly exhausted, the pulse excessively weak and quick, the lower extremities covered with petechiæ, heat and restlessness increased; in short, general symptoms of irritation demanded a suspension of the mercurial course, in spite of any aid from opiates, corroborants, &c.

Under these circumstances, I was induced from the character given of nitrous acid, to try its effect. A dram and half was diluted with a quart of water, to which were added, two table-spoonfuls of rum, and as much sugar as he pleased. It proved rather grateful to him than otherwise, and was all taken with ease in the course of the first night. The same quantity was given daily, the next three days, when it was increased to two drams to two quarts. It still agreed with him, and the rum was omitted. No other medicine was given at this time. He drank bottled cyder when thirsty. His diet

consisted chiefly of gruel, with milk, to which he was very partial. In a few days he felt himself generally better, and was able to sleep without anodynes. And at the end of a week, I had the pleasure to observe, on my entering his room in the morning, seven or eight large rummer glasses full of wine discharged during the night, the colour still dark, but more transparent. This was the first material change since the adoption of the acid, and the first that emitted the least ray of hope. From this period, that is, the first week of taking this medicine, and six weeks from the operation of tapping, I found no occasion to omit, or alter the preparation—He took it six weeks longer, daily, evacuating such quantities of urine, as gradually reduced the swellings of the abdomen and extremities to their natural sizes. A free spitting, and the same forencess of the mouth as was excited by mercurials, continued during the whole of this time, and the fœtor of the breath was unaltered. A bandage was now necessary, to support the flaccid muscles of the belly, and pills of cicuta were given on account

of an induration, which would be evidently felt in the region of the liver. A liniment also was rubbed on the abdomen, twice a day, composed of camphorated oil and tinct. opii. With the exception of these remedies, none but the acid had been exhibited for six weeks past. His appetite and strength had gradually improved—his stools were now natural and regular, and he sat up several hours in the day. At the end of the next fortnight, he quitted his lodgings, and returned to his own house sixty miles distant, still taking the acid. About a month after his return, I heard from him, that he had left off the medicine, and was better than he had been for several years before.—I have this day, received a message from him (six months having elapsed since I saw him) that he continues free from swelling, and in all respects better than he has been for years past, and requesting that I will publish his case, not concealing either the name or cause of the complaint, if I think proper.

Observations on the foregoing Case.

Instances of recovery after tapping in drop-fies, are I believe, by no means frequent. But in the present case, where the cause was of such a nature that there could be no doubt about the existence of diseased liver, it is a fair presumption to conclude, that the recovery is attributable to some extraordinary means.

The first important occurrence in the case, is the relief afforded to the stomach, by the action of mercury on the system. It seems that the morbid secretion from the liver, stimulating the stomach and bowels to vomitings and purging, was corrected almost immediately as the mouth became affected. This event held out hopes, that perseverance in the moderate use of this remedy, would produce further beneficial effects. But these expectations were fallacious. At the end of three weeks we found him in a state that forbid the process, and by the

surgeon of his ship, as well as other medical gentlemen, who saw him, it was concluded, that his disease was beyond even the palliative powers of medicine. He was in this state when the nitrous acid was first employed. Its effects are already detailed, and in offering them to the public, I pledge myself for a rigid adherence to facts.

I shall leave the reader to form his own inferences from this case, but must take the liberty to state how far I conceive my patient indebted for his recovery, to the use of diluted nitrous acid. Although the previous exhibition and effects of mercury deprive the acid of an exclusive or specific claim to the salutary change, no man will I conceive deny, that it proved a valuable resource at the moment it was first used. I am strongly impressed with its merits, and may be inclined to speak of it with partiality ; but taken even in a secondary point of view, as an auxillary to the work begun by mercurials, it claims the attention of practitioners in general, on similar occasions. If there exists another medicine capable of performing

what it effected, viz : preserving the same beneficial change as at first resulted from mercurials, keeping up the salivation, while it improved the appetite and increased the flow of urine, so as gradually to remove all the dropfical symptoms, I was and am ignorant of it. Without it, I cannot say positively what the event might have been, but the prospect was gloomy, and the issue would have been from all appearances fatal.

If these facts though in a solitary instance, can give any character to the nitrous acid, in the removal of dropfy, the ratio medendi offers reflections perfectly consistent with them. Indurations, or some morbid state of the liver, have been found productive of, or connected with, dropfy in general. If then, there be any truth in the repeated assertions of modern authors, that nitrous acid is specifically useful in diseases of the liver, the inference is obvious. From this consideration I was induced to employ it; the event has justified the experiment.

Note from Mr. SCOTT, of Bombay.

I fear that both you and the public are tired of me, but I think (excuse my vanity if I be wrong) that I have still something new and useful to communicate. I have written another letter on the effects of the nitric acid bath on the human body, which I shall send you when opportunity occurs by sea, but it is greatly too long to forward by this dispatch which goes (*viâ Buffora*) over the great desert to Europe. If you think proper to publish the following paragraphs I shall be happy, as it may induce practitioners in Europe, to try the nitric acid bath from which I have experienced some very good effects. I write to nobody but yourself at present on this subject.

“ Since I wrote you last, we have made a good many trials of the nitrous acid bath, which have confirmed the hope that I expressed some time ago of its being an useful remedy for some diseases. By being applied to the surface

of the body, I find that the acid, as I supposed, is absorbed very plentifully by the skin, and that it performs the same effects in the system which arise from its internal use. I have in many cases, immersed the whole surface of the body below the chin for half an hour daily, and with some people for twenty, or five and twenty days successively. I make the bath so strong with acid as to irritate the skin to a certain degree on which perhaps its absorption depends. In about a week it brings on with many people a ptyalism and a soreness of the mouth and throat. It affected me, and it has others, in still a shorter time. These symptoms are accompanied or succeeded by an increased quickness of pulse. I have seen it produce as violent a salivation as ever I saw from mercury, but I think this is not attended with the same kind of fœtor of the breath. By merely immersing the legs in the acid bath for half an hour daily, I frequently see a ptyalism come on and continue while the bath is continued. Some people seem to be nearly insensible of

these effects in whatever way the acid be applied."

" The acid bath if managed with any kind of prudence, is an extremely safe remedy. I have seen it do great good in chronic hepatitis under a variety of forms. I have found it of the greatest service in asthma, and I have used it with advantage in fever. Like the acid internally it has an antisyphilitic power. I have tried it in a number of cases where mercury had entirely failed, and the result of my experience *seems* to be that I have cured two or three people under those circumstances. In several instances I have seen no kind of relief from it, but in a much greater number I have found it like mercury keep away the pains, &c. for a time, but they have returned again on leaving it off. It is well deserving of the attention of practitioners, but my experience of it in this disease, is for too short a time and to too small an extent to enable me to speak with the precision that I desire. We have tried it too in cases of recent syphilis for chancre and bubo. Under such circumstances it seems to succeed

very well. It may be supposed that this success arises from its internal action and that a relapse is to be feared. This remains to be determined. Of all the means that I have seen of getting rid of those symptoms, this is the least injurious, the most agreeable, and sometimes the speediest. When it affects the mouth, I should hope that the cure would be permanent : but allow me to say again, that I do not pretend to determine the point. Let the bath then in those cases be employed with doubt and with prudence, but let it not be rejected entirely ; for if it should be found to answer every intention, it will indeed be a blessing to all mankind. I should think that public baths of this kind in London, would be of great benefit, and would in time, very well answer the end of any individual who should institute them. Such an establishment would already find employment in this place."

" I conclude, by saying, that I am far from pretending to ascertain the extent of the powers of the nitric bath as a remedy for disease, but I am certain that it is very considerable, and you

may now *meo periculo* publish this as my opinion."

Several surgeons here, have promised to keep an account of the cases in which they employ the bath, and which I hope to send you by the ships of the season. I beg of you to try the nitric acid internally in dropsy. If your ascites arises from the same causes with that of this country, I promise you great success.

Cordially wishing you success in your efforts for the progress of knowledge,

I remain, &c. &c.

Bombay.

W. SCOTT.

Dr. BEDDOES.

This note arrived in a letter, dated Nov. 2, 1798. I give it to the public without any comment except a reference to my *collection of testimonies on nitric acid* (Johnson, 1799;) and the expression of my belief, founded on some

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observation, that Dr. Scott and Dr. Luke are right in thinking that nitrous acid will be highly useful in dropsy.

EDITOR.

An Account of several Veins of Sulphate of Strontian or Strontites, found in the neighbourhood of BRISTOL, with an analysis of the different varieties.

By WILLIAM CLAYFIELD.

The first specimen of sulphate of strontian was shewn me by Mr. Tobin, about three years since. At that time it was generally believed to be merely a variety of sulphate of barites. It had been found at Redland a short time before, in a vein of considerable thickness.

The greater part of this vein has received a red tinge from the iron-stone on which it lies, and exhibits but slight traces of any regular chrySTALLIZATION. In some few situations however, it is entirely free from color, and appears to be composed of a confused mass of bevelled tables, loosely adhering together. Its specific

gravity varies from 3. 51. to 3. 87. Walking along the beach at Aust-Passage, in June 1797, I met with a similar substance, and soon discovered several detached veins, in different parts of the cliff. The strata in which these veins are found, are nearly horizontal, consisting of lime-stone, of different degrees of hardness, and argillaceous sand-stone, intermixed with clay and gypsum. The whole cliff as well as the surrounding country, have evidently been produced by aqueous deposition, since which period, the level of the water in the main channel having been considerably lowered, the Severn current has acquired sufficient force to deepen the bed of the river, by plowing up the strata, which had been previously formed.

A second deposition of the soil towards the mouth of the river, by forming diagonal fissures in the cliff, has occasioned an inequality of five or six feet in the level of the strata.

These fissures are mostly filled up with veins of strontian, from three to twelve inches in thickness, consisting of an assemblage of semi-transparent crystals, flanked up on each side

by a thin layer, of a fibrous fracture ; both of a delicately blue tinge. This last variety was observed about three months since, by Mr. Deriabin (inspector of the Russian mines) who was immediately struck with its resemblance to a substance of the same nature, found in Pennsylvania.

The chrySTALLIZATION of the middle vein is either that of bevelled tables, or rhomboidal cubes, of nearly an inch in diameter, the transparency of the latter exceeds that of every other species ; the specific gravity of the cubes varied from 3. 88. to 3. 96, while that of the fibrous was about 3. 91.

Wishing to obtain some muriate of barites about nine months since, I reduced a portion of the spar to the state of a sulphure, and dissolved the earth in marine acid ; the great solubility of the salt, with its needle-formed chrySTALS, soon indicated the presence of strontian.

Several trials which were then made with it, fully confirmed the result of the first experiment. Shortly after this, Dr. Beddoes informed me,

of his having met with a paper of Klaproth's, containing an analysis of the American sulphate.

Since the first discovery of this rare production, Mr. Bright has furnished me with specimens of another variety, from the neighbourhood of Ham-Green, where it is found breaking through the soil in such large masses, that it has been made use of in mending the roads. The chrySTALLIZATION of the latter, like that of the Redland, consists of bevelled tables; it does not however, partake either of its tinge or semi-transparency; its specific gravity is between 3.60 and 3.68.

The present state of the arts furnishes continual instances of the refuse of one manufacture forming the basis of a second. While this continues to take place, it is evident, that every new production must claim a full investigation. The peculiar properties of this earth, render it probable, that its affinities may shortly be made to furnish us with those productions from the raw materials of our own island, which we can now only obtain with considerable difficulty from other countries.

The following analysis was undertaken at the solicitation of some chemical friends ; more leisure would doubtless have contributed to greater accuracy. What is now stated is the mean result of several experiments, the differences of which have rarely amounted to more than two or three grains. Considering the fibrous variety as the most deserving attention, it was the first subjected to analysis.

To find whether it contained any portion of water or other volatile material, 500 grains were exposed to a red heat, under a muffle ; the loss amounting to no more than four grains, proves that the quantity of water, if any, must have been very trifling.

1. 200 grains of the powdered spar in its original state were digested with a solution of carbonate of potash (obtained by deflagrating nitre and tartar) ; the powder, when dried in a red heat, weighed 163.5 grains.

2. A solution of this powder in diluted marine acid, extricated 47 grains of carbonic acid, leaving about 1 grain undissolved : this was afterwards taken up by the alternate appli-

cation of carbonate of potash and marine acid. From this it appears that the whole quantity of earth must have been very nearly 116.5 grains.

3. The solution No. 2 was then fully charged with caustic ammoniacal gas, which produced scarcely any traces of precipitation; the addition of carbonate of ammonia immediately threw down a precipitate, which dried as before, weighed nearly 160 grains; the difference in the weight of the precipitate, and that of the 162.5 grains taken up by the marine acid, arose from decanting the solution into different vessels.

4. To detect any small portion of barytes which might be contained in the precipitate, a quantity of marine acid was poured on it, sufficient to dissolve only a few grains of the earth. Had any barites been present, it would have been taken up in preference to the strontian, from its superior affinity for the acid; the solution, however, after digestion for several hours, still crystallized in needles, and afforded a copious precipitate to baritic lime-water.

5. 164 grains of the precipitated carbonate of strontian were thrown into marine acid, which

extricated as before, about 47 grains of carbonic acid, leaving nearly 1 grain undissolved; the addition of sulphuric acid to the solution reproduced 200.8 grains of sulphate of strontian.

6. The solution of potash No. 1 was then taken, and the whole of the carbonic acid remaining in it disengaged by an excess of marine acid; the addition of muriate of barytes afforded a precipitate, which after drying in a red heat, weighed nearly 249 grains; had the whole of the 200 grains been decomposed, the solution would have furnished nearly 250 grains of sulphate of barytes.

Klaproth and Dr. Withering having estimated the quantity of sulphuric acid contained in artificial sulphate of barytes at .33, and Fourcroy at .35 of the specific gravity of 2.24, or that contained in sulphate of potash; before the concentration of the acid contained in sulphate of strontian could be known, it was necessary to make the following experiments.

7. 218.5 grains of artificial sulphate of barites, were decomposed by digesting with a solution of carbonate of potash, producing 190 grains of carbonate of barites, from which ma-

rine acid separated 42 grains of carbonic acid, leaving nearly 148 grains of earth in solution; from this expt. it appears, that sulphate of barites contains about 32.2. per cent of acid.

8. To find the concentration of this acid, 124 grains of sulphuric acid of 1.843 specific gravity, containing (according to Kirwan's table in the Irish transactions) 109.12 grains of standard, or 97.42 grains of 2.24 were precipitated by barytic lime-water, producing 283.3 grains of sulphate of barities, containing nearly 34.4. per cent of acid of 2.24 specific gravity.

9. 92.2. grains of the same acid were precipitated by a solution of muriate of barites, the sulphate of barites weighed nearly 212 grains, containing about 34.1. per cent of acid of 2. 24.

By taking the mean of these experiments, we may estimate the quantity of acid contained in sulphate of barites at 33 per cent of the specific gravity of 2.24.

According to this calculation, the 250 grains of sulphate of barites, No. 6. would furnish 82. 5. grains of acid of the above strength.

10. To ascertain the difference between native and artificial sulphate of strontian, 204.2.

grains of sulphuric acid of 1.843 (containing 160.44. grains of 2.24. specific gravity) were precipitated by strontian lime-water, producing 360 grains of sulphate of strontian, containing about 44.5 per cent of acid. This accounts for the 200.8 of sulphate being produced from 163 grains of carbonate of strontian, No. 5 ; hence the proportion of acid in the artificial, will exceed that of the native nearly 2 per cent.

11. To determine whether the solution No. 2, contained any calcareous earth, a small quantity of oxalic acid was added to it ; no precipitation however took place.

12. Prussiate of potash occasioned a flight blue tinge.

The different varieties containing so nearly an equal proportion of earth and acid, the statement of a single analysis will be sufficient for the whole. Should there be any difference between them, it will probably be found in the Ham-Green variety's containing rather more acid : the quantity of sulphate of barytes produced from it amounting to nearly 252 grains.

From the foregoing experiments it appears,

that 200 grains of the fibrous variety contain

Strontian	-	-	-	-	116.5
Acid of 2.24	-	-	-	-	83.5
With a small proportion of Iron					
					200.

In addition to the modes of distinguishing the two earths already noticed, we may state the crystallization of the sulphures.

A warm solution of the sulphure of barytes deposits on cooling, an assemblage of several very thin layers of inclined oval plates, terminating in points, and radiating from a center; while the sulphure of strontian runs into a base line, supporting a number of parallel perpendiculars gradually lessening, so as to form the diagonal of a square.

The strontian earth in a state of purity, frequently varies in its crystallization, sometimes depositing solitary tables, and at others arranging them in regular lines.

Both barytes and strontian combine with phosphorus, and exhibit similar appearances to the phosphure of lime. Mixed with a few grains of oxygenated muriate of potash, and triturated in a mortar; an explosion took place.

Note by the EDITOR.

Mr. Clayfield has lately been informed that another variety of fulfate of ftrontites is found near Sodbury. Mr. Deriabin has feen a blueifh fibrous variety from a coal pit near Dumbarton. On a professional jonrney to the North, I was ftruck at Kefwick with a fpecimen in Mr. Hutton's collection, labelled *ftriated gyffum*. It is an exceedingly beautiful white fulphate of ftrontian from Alfton, as the label bears. From Mr. Hutton I have alfo received a blueifh fpecimen chrystallized in rhomboidal tables which I took for fulpate of ftrontiap, but Mr. Clayfield finds it to be barytes. It comes from Cleter Moor, Cumberland. I have another from Newlands, Cumberland, having the exact appearance of the fulphate of ftrontian from Ham Green, which requires further examination. Many fpecimens, fupposed to be barytic, will doubtlefs, on examination, prove to be ftrontitic. But the diftinction will require nice infpection, even from thofe moft verfed in the external characters of foffils.

The following experiments were made at my request by a friend.—12 grains of carbonate of barytes were given to a two months old rabbit. In half an hour not much affected—in an hour nearly dead. The barytes had acted violently as a cathartic, and had produced almost general paralysis. In about two hours the rabbit died much convulsed. The stomach was greatly inflamed—the inner coat separated from that below, lying in folds, and as if half macerated. 5 grains of barytes in a second experiment, and 2 in a third, killed similar rabbits, with the same effect on the stomach, only the fæces were not so much softened. 40 grains of sulphate of barytes had no effect. 12 grains of carbonate of strontites, obtained from the Bristol sulphate, acted as a cathartic only; of the sulphate itself 12 grains had no effect. This seems to shew that strontites might be tried with little risk as a medicine, if any analogy should afford hope of benefit from it.

At my further request, the following experiments were tried. 5 grains of carbonate of barytes, mixed with 5 of Cayenne pepper, were

given to a similar rabbit, which lived five hours and was not much affected till within an hour of its death; when it suffered as in the first experiment, and the stomach was similarly injured. Considering that sulphate of barytes has no action, by reason probably of its insolubility, I thought it possible that innoxious absorbents might engage the acid of the stomach, and prevent the effect of carbonate of barytes; in which case, under certain circumstances, we should be provided with an antidote for this deadly poison. The following experiments shew the conjecture to be erroneous: 5 grains of barytes, with 5 grains of prepared potash, were given to a rabbit. The animal seemed immediately affected, in less than an hour was scarce able to move, and in less than two hours died convulsed, with a stomach extremely injured as before. 5 grains of carbonate of barytes, with 20 of chalk, destroyed a rabbit in five hours. The animal died tranquilly. Carbonate of barytes 5 grains, with olive oil, killed a rabbit in less than two hours. The animal having remained all night unopened, the inner

coat of the stomach was so much loosened as to be shaken out with its contents. Happening to try calcareous liver of sulphur as an antidote, my friend observed the following extraordinary phænomena—

4 grains of carbonate of barytes, with a tea-spoonful of a solution of sulphure of lime being given to a rabbit, two months old, the animal died immediately. The stomach being instantly examined, was found of a dark colour, and at its curvature (qu. *the great curvature?*) converted into a yellow spongy substance, which looked as if burned by a caustic, and when removed left but a very thin membrane. The contents of the stomach near the darkened part, which seemed bounded by a whitish line, were also dark coloured. The *whole* inner coat was destroyed. As this rabbit had two days before been subjected to an experiment with sulphate of barytes, it was necessary to ascertain whether this had not had some share in the unexpected effect. A tea spoonful therefore of the same solution of calcareous sulphure was given to a full-grown rabbit. It had the same immediate

effect in destroying life ; and the state of the stomach was found to be the same. On the 26th of January 1799, a piece of solid sulphure of potash about the size of a pea was given to a rabbit ; and an hour afterwards another piece of the same size. A few minutes after the first dose, the animal had convulsions of the skin, which were considerably increased by the second. The whole head then began to swell, and in two hours the tongue was so enlarged as to be frequently bitten in attempts to masticate. In two hours and a half the swelling was so large, and the breathing attended with such difficulty and croup-like noise, that it seemed probable the animal had but few minutes to live. The operator, heartily sick of these cruel experiments, and unwilling to sacrifice more victims, gave the rabbit two tea spoonsful of olive oil, and was much pleased to find, that in ten minutes it appeared much relieved, and that the noise in respiration had ceased. The animal remained very still, and after oil had been applied to the swellings, was left all night in a basket of hay, in a warm situation.

Jan. 27th. The rabbit had left his basket. A little warm milk was given him. In the evening, he eat his parsley and drank his water heartily. On the evening of the 28th, the power of the antidote being supposed to be ascertained, the rabbit was killed, and a surgeon was desired to examine the swellings. He found that they arose from an enlargement of the maxillary glands and of the lymphatics. The stomach was covered with a number of small specks, and seemed a little softened.—It should be observed, that this rabbit had been kept on dry food, which may account for the comparatively slow action of the sulfure. An accident confirmed this. In the course of the experiment, having put his nose into water, he received some on his tongue, which being swallowed, greatly increased his agonies. It happened shortly before the oil was given. The sulfure had been carefully prepared. The experiment with the solution of calcareous sulfure, was twice repeated. Each time the rabbit died in less than two minutes.—The stomach was black and yellow, spongy, and much corroded.

On the WHITENING of BONES.

The best method of whitening bones, has long been a desideratum, upon which I have bestowed no little attention.—The usual custom of exposing them to the action of the sun and air, will, under certain circumstances, render their appearance extremely beautiful ; but this, in large cities, where the atmosphere is loaded with foot, is at best, very tedious, and frequently altogether impracticable. Washing them with muriatic acid, and soaking them in lime and alum-water, have each in their turn, been strenuously recommended. I have tried each, and have in some instances, succeeded tolerably well ; but I never saw effects produced, sufficiently striking, to induce the adoption of either plan, to the exclusion of the rest. Bones are sometimes whitened by being boiled for a length of time in water, saturated with kali. This plan is however, a bad one, for if the boiling be continued till the oil is *perfectly* extracted, they

will be so soft, as to be unfit for demonstration; and in the other case, they will retain their oily-brown appearance. This I learnt, after having sacrificed two sets of finger and wrist-bones in experiments. It must be understood, that previous to any attempts to bleach them, all extraneous dirt, and putrid soft parts, had been compleatly washed away.

I am acquainted with only two methods of removing the integuments.

1st. By covering the limb with water in a convenient vessel, and suffering it to remain in that situation for several months, when the muscles, tendons, &c. will become a soft putrid mass, from which the bones must be separated by repeated washing. .

2d. (Which I consider as the less eligible of the two) consists in exposing the parts to the action of potash, rendered caustic by the addition of lime.—This is, however, very convenient, where dispatch is necessary, as eight or ten hours, are in most cases, sufficient to effect the purpose. In my father's collection, is a carious tibia, prepared in this manner,

which is by no means among the least valuable specimens in his museum.

In the spring of 1798, I was struck with the wonderful effects produced by the bleaching liquor of the cotton and linen manufacturers: It immediately occurred to me, that its action upon bones was worthy of investigation; and a favorable opportunity of trying it, soon after presenting itself, I was so convinced of its efficacy, that I immediately fitted up an apparatus for the purpose.

I had at this time, a cranium in my possession, lately prepared, perfectly clean, and inodorous, but so brown, that I had thrown it aside, as unworthy a place in the museum: This, (together with the vertebræ of the neck) formed the subject of my first experiment. It was evident, that every good purpose might be answered by exposing the bones to an oxygenated muriatic atmosphere merely; and in order to accomplish this, I adapted a cork with a stop-cock driven through it, to the aperture made for the candle, at the bottom of a common lamp-glass. A Florence-flask which contained a

quarter of an ounce of *black calx of manganese*, mixed with half an ounce of *muriatic acid*, served for a retort, and a long-bent tube of glass fixed into its neck, enabled me to direct the product at pleasure.

The skull had been immersed for twelve hours, in a weak caustic solution of potash; but whether this is absolutely necessary, I have not yet ascertained.

The lamp which contained the head and several other bones, previously prepared in the same manner, was placed in a large trough of water. The stop-cock at the end, afforded me the means of filling it with water, by exhausting the column of air above. Every thing being thus in readiness, it remained only to dislodge the water, by placing the flame of a spirit-lamp under the retort, and the extremity of the tube under the lamp-glass. In a few minutes, the yellow oxygenated muriatic acid gas, was evolved, and I perceived, that as the water sunk, those bones which were exposed to its influence assumed the same appearance.

The operation was continued, till the glass

was emptied ; but I found it necessary to continue the process slowly, in order to recruit the air which had been absorbed by the preparation and water ;—a circumstance which was demonstrated by the rising of that fluid.

In six or eight hours I removed the glass, and exposed the head, which was of a bright golden colour, to the open air, and the rays of the sun ; as it dried it became paler, and covered with small shining crystals (which were afterwards washed off in rain water), and I had soon the pleasure to find it so beautifully white, that it exceeded in appearance every thing of the kind which I before possessed. The dark case in which it has been some time enclosed, has rather diminished its whiteness ; a circumstance which usually takes place where the light is excluded : but it still retains its beauty sufficiently to demonstrate the superiority of this process.

With old bones it does not succeed so well as with recent ; but even these, in some cases, were well bleached.

When the above experiments were made, I had not the least idea that they would ever

meet the eye of the public, consequently so much attention was not bestowed upon minutiae, as would otherwise have been the case. If, however, you consider this communication worth preservation, it may possibly excite the attention of anatomists, to a branch of science at present in its infancy.

I remain, sir, &c.

Bristol, Mar. 2, 1799. RICHARD SMITH.

Dr. BEDDOES.

*Letter from Mr. ———, Surgeon of ———
Hospital, on Gonorrhœa.*

Dear sir,

I present you with the two following cases of gonorrhœa, which were treated with the muriate of quicksilver, according to the method recommended by your correspondent Mr. Addington, excepting the use of the glauher's salts, which at your request were not exhibited. These two cases I purposely selected, as one of

them was of very long standing, the other of a recent date ; and both the patients otherwise in good health. I am sorry to say the result, as will appear on consulting the cases, was far from flattering ; the medicine, which was faithfully prepared and administered, was taken for the few first doses without producing any alarming symptoms ; but as soon as I administered it in the form recommended, viz. a grain and half of muriate of quicksilver, dissolved in half an ounce of rectified spirit of wine, such violent symptoms in both instances, and in Holt's case such dangerous ones supervened, without any amendment in the disease taking place, that I should not have held myself justified in persisting further in the use of the medicine.

Supposing cures to have been accomplished in almost every instance in which these large doses of muriate are said to have been given, as these cures could only have been accomplished by the violent local action which the medicine had excited on the mouth, fauces, throat, and stomach, suspending and annihilating the diseased action in the urethra ; (for it appears to

me to produce an action in the stomach, throat and fauces, sufficiently violent to remove the distant diseased action of the urethra,) it must be attended with so much hazard to the patient, as cannot warrant its general use; though it be granted that troublesome cases of gonorrhœa now and then present themselves to our notice, in which strong means are obliged to be had recourse to. Had I not placed the greatest confidence in your correspondent's detail, I should not have had the courage to have given so large a dose of muriate of quicksilver, especially when I reflected on two cases which have occurred in this hospital during the war, in one of which dangerous, and in the other fatal consequences, followed the exhibition of the muriate, though taken in smaller quantities than is now recommended in gonorrhœa.

The testimonies offered by Mr. A. are so strong, that I hope the hints and failures which I now send, will not deter nor prevent other practitioners from entering on the use of this medicine, in whose hands I trust it will be more fortunate.

I am, &c. — —

JOSEPH HOLT, a strong athletic seaman, was admitted into the _____ at _____ on the 30th of January, 1799. He said, that about fourteen months before, he had contracted a gonorrhœa, which, according to the description of the symptoms he then suffered, could not have been very virulent : that a week after its appearance he made application to his surgeon, under whose charge and care he continued upwards of three months, at the expiration of which time, the disease instead of being cured had gained ground, in spite of various means which the surgeon had with great attention employed : that he then returned to his duty, with a discharge from the urethra, which always encreased on any excess being committed. About ten days before his admission here, a new surgeon having been appointed to his ship, to him he made application : though he also exerted his endeavours, no amendment took place. When admitted into this hospital, I found that a constant and copious discharge of purulent matter flowed from the urethra, which was very tender, more par-

ticularly so about two inches from the orifice. The discharge was similar in appearance and consistence to that which is generally observed near the termination of gonorrhœa; he experienced now no ardor urinæ, no chordee, nor any inflammatory symptom; neither was there the smallest reason to suspect either a diseased bladder, prostrate gland, or strictured urethra: the patient's spirits were apparently oppressed from the duration of the disease, and he was very anxious and willing to enter on any plan that could be devised for his cure. January 31st
 R hydrargyri muriati gr. i. sp. vini rectif. ℥ i. misce et adde syrup. simplic. ℥ ij. aq. menth. pip. ℥ i. m. pro. haust; which was given about noon. In about ten minutes a frothy salivary discharge (such as we observe flow from the mouth of a dog when much exercised in warm weather) was excited, and continued two hours; quantity spat about a quart. Feb. 2d, a grain and quarter of hyd. muriat. mixed in the former manner was given; it produced the same frothy discharge as before, but it lasted only one hour. 4th, a grain and half of hydrarg.

muriat. compounded as before, was taken, which soon produced so much uneasiness in the stomach, as nearly caused vomiting, and obliged him to go to bed, which had not happened with the other doses; spat but little. In about three hours he had several discharges from the bowels, after having endured much griping pain. 5th. This morning the patient declared that both the discharge and pain of the urethra were not "half as much" as they had been before coming to the Hospital. 6th. Repeated the draught as on the 4th, which produced efforts to vomit, with griping pain of the bowels; it did not, however, oblige him to lay down; spat little. 8th. The discharge from the urethra was not lessened; the draught was repeated; spat about twenty minutes; it produced neither nausea in the stomach, nor uneasiness in the bowels. 10th. Disease rather better; repeated the draught, which excited more spitting than usual, and was followed in a few hours by ten discharges from the bowels, without pain. 12th. Gonorrhœa as at first; repeated the draught, which produced an incli-

nation to vomit, which was allayed by walking out into the open air ; spat about ten minutes. 14th. The following draught ; $\mathcal{R}$ hydrargyri muriat. gr. iſs. ſp. vini rectif. $\mathfrak{z}$ ſs. m. was given, which on ſwallowing produced a violent pungent heat, followed immediately by ſpitting, which went off in about eight minutes ; he was then compelled to ſeek his bed from the tormenting pain which came on in the fauces, ſtomach, bowels, and head ; his bowels and head he deſcribed as on fire ; ſevere vomitings and ſtrainings ſupervened ; the ſtomach rejected every thing for the three following days. On viſiting him again about eight hours after taking the muriate, I found that theſe ſymptoms were much aggravated, with a full, quick pulſe, a dry furred tongue, a great confuſion in his manner and anſwers ; the abdomen much tumefied, which was relieved by prodigious evacuations of flatus by the mouth and anus : theſe alarming ſymptoms in four days were ſubdued, but not till after much real anxiety had been experienced on my part, and ſevere pain and ſuffering on his. The diſcharge from the urethra,

and other symptoms, which were certainly at one period better, were now as bad as before we entered on the use of muriate of quicksilver.

MICHAEL M^C. CUE. a Marine, twenty-one years of age, was received into the ——— at ———, on the 31st. of January, 1799. having about six weeks before, contracted a gonorrhœa, for which he had taken a few powders and some mercurial pills, without producing any amendment. The day of his admission hither, I found that he had a gonorrhœa which discharged freely, attended with ardor urinæ—a pouting of the lips of the urethra, with inflammation of the glans—a troublesome chordee, and an almost constant inclination to void his urine.—Feb. 1. I gave him the following draught : R hydrargyri muriati gr. i. sp. vini rectif. ℥i. misce et adde syrup. simplic ij. aqu. menth. pip. ℥i. m. pro. haust. In about fifteen minutes after taking it, a frothy salival discharge came on, and continued two hours ; in which time, nearly a quart was discharged, producing no other effect.—3d. The gonorrhœa

was not better—a grain and quarter of hydrarg: mur: was given in the same form, which produced uneasiness in the stomach and bowels. Spitting was brought on in about ten minutes, and remained an hour. Quantity spat, about a pint.—5th. The ardor urinæ, and chordee, were lessened. The frequent inclination to urine, was gone off: a draught, containing a grain and half of hyd. muriat. mixed as before, was taken—In twelve minutes, a spitting was excited, and lasted an hour. The draught produced much pain in the stomach—general health was good.—7th. Repeated the draught as on 5th. It produced great inquietude, and immediate spitting, which subsided in about an hour. In the night he was much griped. The chordee returned with more violence, and the other symptoms were as bad as ever.—9th. The gonorrhœa was not better—the draught was repeated: spitting came on directly, and continued about forty minutes; quantity discharged, about a pint: no bad effects followed.—11th. Repeated the draught: spitting immediately took place, and went off again in about half an

hour : quantity discharged very small : gonorrhœa not mended.—13th. Gonorrhœa worse. I gave him a grain and half of the muriate of quicksilver, dissolved in half an ounce of rectified spirit of wine, which made him urge to vomit, and excited spitting the instant it was swallowed, which did not go off for three hours ; during which, he spat about a pint and half.—So violent a pain came on over his eyes, as forced him to bed. Afterwards, he was much purged.—15th. Repeated the draught of the same form and strength as the last—spat about a pint—vomited with great straining for six hours ; complaining of a burning pain in his mouth, throat, and stomach.—16. The gonorrhœa with all its attendant symptoms, being aggravated, it was not deemed prudent, or justifiable, to persevere in the further use of the muriate of quicksilver.

Letter from MR. ADDINGTON:

WEST BROMWICH, *March 5, 1799.*

Dear Sir,

I beg of you to accept my best thanks for the printed sheet which you was so good as to send me on Saturday, for your letter of the next day, and for the remainder of the reports on nitrous acid, received through the kindness of Mr. Watt, a few days ago. On the use of this acid in the treatment of syphilis, I have only now to add to my former testimony, that the disappointments I then met with, have since prevented my relying upon it *solely* for a cure; but that I have, as far as I can judge, found it a very useful *auxiliary*.

With respect to the treatment of gonorrhœa by muriate of quicksilver, I can now inform you that the practice, since the date of my former communication, has been generally satisfactory; as I have no doubt would be shewn by a particular detail of the cases that have arisen, did the time in which it is necessary to send you

this reply admit of its being made out. As this is impossible in the few hours allowed me, I must confine myself at present to a few remarks on the two cases contained in the sheet you have had the goodness to send me. These remarks are so perfectly obvious on even a cursory view of the treatment in these instances, compared with that which was employed in the cases I had laid before you, that they may be supposed to occur to every reader of both. Having however been the first to adopt, and as far as my communication goes, to recommend the practice referred to, it may perhaps be incumbent upon me to take some notice of its progress. In this view I am by no means sorry for the publication of the unsuccessful cases of Holt and M^c. Cue, transmitted by your correspondent, as they may serve a valuable purpose in guarding and directing our future practice in the employment of a medicine of such confessedly active powers. *Three* things are very observable in the statement of your correspondent, as material deviations from the plan I had followed: viz. first, the omission of the

glaubers salt after every dose of the muriate. Secondly, the very short intervals at which the latter was repeated; and thirdly, the great number of doses given. The *first*, it seems, was by your direction, and I leave it to you to consider what may have been its influence on the very different results both as to the failure of the expected good effects, and as to the violent and deleterious operation of the latter disease. If, as I have for the most part supposed, this part of the prescription be directed less against the disease, than against the intervention of any pernicious effects of the muriate, the omission of it may powerfully combine with the *second* particular of deviation above mentioned. In the transcript of cases which I sent you, it will appear that a clear interval of two or three days was usually allowed between the several doses of the medicine, even the two or three first doses; and when more than these were required to accomplish the cure, the length of interval was extended, sometimes to four, five, or six days, or more. There is an exception to this rule at case 10th, which was owing to some

circumstances which rendered expedition in the cure particularly defirable to this patient, and I observed in him a remarkable insensibility, even to the *disagreeable* sensations which are usually attendant on the immediate operation of the medicine ; and farther, it is to be observed that the salts were always given on the intervening days. The *third* particular which I have noticed as a deviation, consists in the number of doses given ; for which it may be alledged, a necessity was created by the inefficacy of the medicine in the early exhibition of it. To this I have only to reply, that I have not found such a number of doses necessary in any instance hitherto. The disease has sometimes been subdued by two of the draughts, most commonly by three or four, and six is the greatest number given. In one of the instances, viz. Case I, I have stated that I believed the two, if not three last, to be wholly unnecessary ; and in the other, case VI, it is remarkable enough that *all* were ineffectual. But too much stress cannot be laid on the observation that the temporary advantage which your correspondent

states to have been obtained even in these two cases, followed upon the *early* doses—one patient it seems declared after the *third* that both pain and discharge were lessened more than “*one half*,”—and farther, that the violent and deleterious effects took place after the latter or last of the *eight* draughts which were administered in this quick succession of every other day, without any intermediate precautionary measures. It is curious also to perceive, that when these severe and dangerous affections of the system thus supervened, the disease, so far from being lessened in proportion, actually regained its former ground and strength; a fact, which at least affords no support to your correspondent’s supposition of the *modus operandi* of the medicine, and the hazard that must attend its general use. I have had occasion to remark, that when the *stomach* and *bowels* have partaken most of its action, and vomiting and purging have been induced, its beneficial effect on the urethral disease has been less considerable than when its operation has been more confined to the fauces and

mouth, with the salivary glands.—I am not inclined however, even if the time permitted, to enter into any argument on the subject; with the few facts yet obtained it would be premature. No one can entertain more serious apprehensions than I did on the first view of the prescription, nor enter on the exhibition of the medicine with more timidity or reluctance: with respect to the cases already given, I can only aver, that they are faithfully related; and may now add, that in perhaps as many more which have since occurred, I have never once met with any of those violent affections of the system which took place in Mr. ———'s two patients. I have before said that the success though not uniform, has proved generally satisfactory.

I shall continue to record the practice with as much accuracy as I am capable of, and in the mean time shall be glad to observe the degree in which it succeeds in the hands of other practitioners who may be disposed to subject it to the test of experiment under their own observation.

I remain, dear sir, with true respect,

J. ADDINGTON,

E e

Note by the EDITOR on the use of mercury in febrile diseases. (See above, p. 377, Mr. HAMMICK on Dr. GEACH's practice).

There seems every reason to suppose that mercury has continued more or less in use in fevers and in pyrexia, since the rise of the alchemical sect, or before. In gout and rheumatism, and other complaints, it is much recommended in F. Hildanus, and Zac. Lusitanus. Boerhaave thought highly of it in small-pox: English medical writers, a century ago, in inflammations. Its external and internal use in catarrhs, inflammations and proper fevers grew very common among Italian practitioners in the earlier half of the present century, as may be seen at large in Rotario *remedio alle catarrali molestie e a qualsivoglia infiammazione*, Verona, 1733, and in Moreali *Systema februm malignarum Mutinae* 1739. Some time afterwards we find bleeding and mercury employed in fevers in Italy full as freely as of late in the W. Indies, or in America, only that bark was sometimes

largely added to the mercury. Benvenuti *disf. quâ epid. febres describuntur, necnon et cort p. usus Luccæ* 1754; describes a fever, attacking particularly full and robust young people. Dissection shewed either inflammation or mortification of the meninges of the brain, of the stomach, bowels or liver. At the onset copious bleedings were ordered, and three or four times repeated. Then lenitive electuary with two scruples of *mercurius dulcis* was given, by which means Benvenuti restored many to their pristine health. When this plan did not answer soon, a dram of bark was added to a scruple of the mercury, and of this powder a scruple taken every four hours for three days, when the fever commonly disappeared. In desperate cases he gave three drams of bark with one dram of the mercury at once. In this way he says he cured many patients: his success he ascribes principally to the mercury; and declares he never observed any bad consequence from the practice. A great deal of information may be found in the following learned dissertation. J. J. Rambach *usus mercurii in morbis inflammatoriis. Halæ* 1794.

NOTE from DR. JENNER,
Respecting the preceeding FACTS on COW-POX.

I feel myself sensibly obliged by the candour, with which you have communicated the letters of Mr. Cooke and Mr. Thornton, relative to the cow-pox and the small-pox. I have neither leisure nor inclination at the present moment to enter into an examination of their arguments, much less shall I attempt to refute the opinions of either of these gentlemen. I only think it necessary to inform the profession, that I have a pamphlet just ready for the press, intended as a supplement to my former publication on the subject of cow-pox, in which I hope I have in a great measure succeeded in reconciling those seeming contradictions which appear in many parts of this interesting enquiry to have puzzled the minds of the public. I shall only at present observe, that nothing either of fact or argument appears in the communications of your correspondents, which

inclines me to alter the opinion I have formed of the nature of these disorders.

On a perusal of the letters of Mr. Cooke and Mr. Thornton, one remark obviously presents itself, which indeed applies to every subject of human investigation.—*That a candid and judicious public should suspend its decisions on any point where its own benefit is concerned, until the whole of the facts and the consequent rational deductions shall be laid before them.*—The same equitable tribunal perhaps, will not fail to discriminate between the man who sedulously employs the greatest part of his time in making experiments for the complete investigation of a confessedly complex subject, and him who appears peremptorily to decide on the truth or falshood of a theory, on the supposed authority of a few solitary instances, which after all may have been mistated or misunderstood.

I remain, &c.

EDWARD JENNER.

no 26th Feb.

To DR. BEDDOES.

*Answers by Mr. JACOBS, Attorney at Law,
BRISTOL, to queries proposed by the EDITOR
respecting the Cow-Pox.*

1. *Are you acquainted with the cow-pox?*

A. The son of a farmer who held a large farm, I was, as soon as able, made to milk, when necessary from other avocations of servants (a large dairy being kept, and in that way heard of what was, and is called, the cow-pox.

2. *Have you had the cow-pox?*

A. Near fifty years ago, the dairy being affected with the cow-pox, I caught it in milking, and others who milked, were infected also. It ran through the dairy, and the disorder might really be called dreadfully troublesome. In the ensuing summer I had a slight infection, and it was partial only amongst the cattle.

3. *How did it affect you?*

A. Sores like the small pox; pustules on every finger; the veins swollen, or much distended, and painful to the shoulder; and the

fingers scarcely to be used, and when used, with much pain, as well otherwise as from the fores. I do not recollect any swelling (besides the distension of the veins) in the arms, but there was a swelling under the arms pretty considerable, but decreasing as the cure proceeded.

4. *Had you general indisposition or fever?*

A. I do not remember any particular indisposition or fever; but herein my memory does not serve me very correctly.

5. *Had you any swellings, and where?*

Answered above.

6. *How long after you had the cow-pox were you affected with the small-pox?*

A. I take it to be near ten years: and that I had been within that period, at various times, in great danger of taking the small-pox from infection, to avoid which I was inoculated in London, and had a great burthen. The first inoculation not appearing on the eighth day to have taken effect, I was again inoculated, and from this had large numbers of pustules on the extremities.

7. *Did you suppose yourself safe from the small-pox after having had the cow-pox?*

A. By no means ; at that time no idea, that I had ever heard of, prevailed, or had been taken up of such an effect.

It is probably Mr. Jacobs, whose case is mentioned in a summary manner at the close of an article by Dr. T. Bradley in No. 1. of the medical journal. The editor conceived it right to determine whether Mr. J's experience actually bore on the great question. The answer to quere 3 seems pretty decisive, especially as Mr. Jacobs appears equally distinct and cautious in what he advances:

A LETTER to Dr. BEDDOES,

Containing observations on the use of Digitalis in Pulmonary Consumption, with two Cases in which it proved permanently successful. By NATHAN DRAKE, M. D. Member of the Royal Medicinal Society of Edinburgh.

SIR,

In a disease so generally fatal as phthisis pulmonalis, and for which, though frequently sought for, no certain remedy has hitherto been discovered, it seems the duty of every intelligent physician to pursue, if possible, an original plan, to ascertain the effects of new medicines, or to reapply those which, though possessing strong powers, caprice, ignorance and apprehension have prematurely laid aside. To your indefatigable perseverance in the cultivation and application of chemical science, we are indebted for a novel class of remedies, which bids fair to remove or alleviate some of the most distressing complaints incident to humanity. To the dis-

case under consideration you have particularly turned your attention, and sought for assistance, not only from pneumatic chemistry, but from every quarter which held forth the prospect of aid. It is therefore with peculiar pleasure that, complying with your request, I now communicate to you two cases of phthisis, in which the *digitalis purpurea* of Linnæus has been employed with permanent success. Though the exhibition of *digitalis* in consumption be not absolutely new, yet I trust the mode in which I have administered it has a claim to that appellation, and the facts brought forward prove, what assuredly is of vast importance, that by the use of this medicine the pulse may be lowered to forty strokes in a minute, without any previous sickness, and the depression continued for weeks together with the happiest consequences.

The fate of this plant has been somewhat singular, for, from the sixteenth century to nearly the present period, it has furnished a subject for applause to one practitioner, and for unqualified condemnation to another. Its earliest describers, however, speak of its success

in pectoral complaints, even Fuchsius (Hist. Plant. 893) to whom we are indebted for its name, and who first introduced it to the medical reader, has not forgotten its utility in these respects. Gerard and Parkinson celebrated it as an expectorant, and Dr. Withering has given us a manuscript note of a Mr. Saunders, found in a copy of Parkinson's Herbal, which mentions consumptions as infallibly cured by a decoction of fox-glove leaves in water. Salmon too, at the commencement of the present century, has noticed it in terms of lavish commendation, and as he affirms his opinion to be the result of repeated experience, it may be worth while, notwithstanding the dash of empiricism which prevades all his writings, to transcribe the passage, "The specific," says he, *Family diet, 4th ed. p. 144*, "which transcends all the medicines here mentioned, and many others besides, is the herb fox-glove. A weak decoction of the herb in water, or in wine, or in half water and half wine, may be drank as ordinary drink ; and of the juice of the herb and flowers may be made a rob or syrup, with honey,

which being taken, three spoons-full at a time, first in the morning fasting ; secondly, at ten in the morning ; thirdly, at four in the afternoon ; and lastly, at going to bed, will restore, where the patient is not past cure, beyond all expectation. It cures a phthisis or ulcer of the lungs, when all other medicines have failed, and the sick are esteemed past cure : but as it is a very strong medicament, and emetic withal, so it ought to be given with discretion, not to transcend the strength of the patient, for then instead of doing good it may do hurt ; and therefore the syrup ought to be taken at first in a lesser dose, and to be increased as you see cause. It opens the breast and lungs, frees them from phlegm, and cleanses the ulcer and heals it when all other remedies act without effect. I have known it to do wonders, and speak here from a long experience. Persons in deep consumptions, and given over by all physicians, have by the use of this herb been strangely recovered, and so perfectly as to grow fat again."

That a medicine so powerfully instrumental in retarding the circulation, so liable to produce

oppressive sickness, together with pain and giddiness in the head, should have been frequently attended with alarming effects, and esteemed by many even as an absolute poison, is not to be wondered at, especially when it is considered that among the poor, where it was at first chiefly used, its incautious exhibition would naturally lead to this conclusion. Even Ray, Boerhaave and Haller, mention its operation as generally deleterious; what however is truly extraordinary, none of the old writers, nor any of the moderns, I believe, previous to the year 1770, have mentioned its peculiar property as a diuretic, confining themselves principally to its administration in epilepsy and scrofulous ulcerations. Its remarkable efficacy, however, in promoting absorption has lately attracted due attention from the medical world, and this, together with its singular dominion over the motions of the heart and arteries induced Dr. Darwin and Sir George Baker to make some experiments with it in pulmonary consumption, and in the third volume of the medical transactions, a case is related by the former, in which

the digitalis was supposed to have operated a cure. In the *Zoonomia*, however, this celebrated physician has since informed us, that the disease terminated fatally. (*Zoonomia* II. 291) Here the medicine was given as in anasarca cases, with a view to promote nausea and consequent absorption; and indeed Sir George Baker, in his appendix to Dr. Darwin's remarks, observes, "if real benefit has at any time accrued to a consumptive patient from the use of this medicine, may it not be conjectured, that it has been, at least in part, effected by the action of vomiting?" The distressing sickness, however, which this plant creates, and the fatigue to which the patient is consequently subjected, led Sir George at the conclusion of his paper to wish, "that some mode or management could be contrived, by the means of which its power of promoting absorption might be exerted, separately from its virulent effects."

Such was the debility of the two patients whose cases are subjoined to these remarks, that I was not willing to risque the consequences of sickness, and became desirous of introducing

the digitalis in so guarded a manner as to preclude, if possible, all tendency toward an affection of the stomach, and yet at the same time of powerfully retarding the circulation, and of promoting absorption from the lungs, and I the more readily conceived these to be attainable, as I had frequently, in the course of practice in anasarca, observed a depression of the pulse and strong diuretic effects from this herb, without any previous sickness. That I should be able not only to produce this retardation of the circulating fluids, but safely also to maintain it for weeks together, was I confess, an event more to be wished for than expected.

As every physician is supposed maturely to weigh his motives for the administration of any medicine, and to form some theory of its operation at least, and probable effects, it may not be unnecessary in this place briefly to state my views in prescribing this plant in cases so apparently desperate.

It has been lately maintained by the most celebrated physiologists, among whom John Hunter stands foremost, that pus is a secreted

fluid, the consequence of certain diseased motions of the extremities of the blood-vessels ; it has been likewise ascertained, that hectic fever arises only from the matter of an open ulcer ; that what is termed laudable pus, when secluded from the air, is neither capable of creating fever, nor, except by its gravity, can it irritate the parts on which it rests. When pus, however, is exposed to atmospheric air, it rapidly attracts oxygen, an acid of a peculiar kind is generated, and hectic fever, the effect of the absorption of aerated matter, is produced. Now as an ulcer of the lungs is perpetually exposed to a stream of air, and of course an ichorous poison continually forming by the union of oxygen with secreted matter, an important curative process would seem to arise from promoting absorption so rapidly from the surface of the diseased parts, that the pus shall be taken up as soon as secreted, and consequently its combination with oxygen prevented. If at the same time the medicine employed to promote absorption, should so powerfully retard the motion of the heart and circulating fluids, that

the irritating and morbid action of the extremities of the blood-vessels, and therefore secretion as its immediate effect, should be considerably diminished, if not altogether suspended, another most salutary purpose would be accomplished. To the cautious and continued use of digitalis, Sir, I looked for these consequences, though whether I should be able to render them sufficiently permanent to promote a cure, was necessarily a matter of great doubt. It was my wish also, as I have mentioned above, to effect these changes without any previous sickness, concluding that should I be able *gradually* to depress the circulation, nausea, as a link in the chain of effects, might be excluded, and absorption, together with a suppression of morbid action and secretion, still be the result.

It is a well-known fact, and probably arising from an indissoluble association between the stomach and the heart, that the pulse generally sinks in consequence of nausea; and as subsequent to the retardation of the action of the heart, absorption frequently occurs. it has been supposed that nausea, a diminution of arterial

motion, and absorption, are mutually and necessarily related to each other, and that were the first of these phænomena abstracted, the latter, viz. absorption, would not be produced. It has therefore been usually attempted to promote pulmonary absorption, through the medium of this affection of the stomach, whence the prescription of emetics, of sailing at sea, and of swinging, to induce vertigo and sickness; and the digitalis has been hitherto exhibited with this view. The cases annexed, however, will prove that the circulation may be safely, powerfully, and perhaps more permanently retarded, independent of any affection of the stomach, and that absorption as certainly follows a depression thus procured, as when sickness has ushered it in.

The preparation of digitalis best adapted to my purpose, appeared to be the *saturated tincture*; and in the first case I commenced with but fifteen drops twice a day, in the second twenty: Mr. Marris gradually increased the dose of tincture until he took one hundred drops: this quantity was first ordered on the

12th of July, when the pulse beat but fifty strokes in the minute, and was continued for nine days, when his pulse dropped to forty. Beyond this depression I thought it unsafe to proceed, and therefore immediately diminished the dose of tincture. During the interval between the 22d of June and the 17th of July, though the dose of the digitalis had in this period gradually attained to its maximum, not the least sickness occurred, nor any one symptom that could lead to apprehension. On the 17th, however, a considerable intermission of the pulse took place, and continued for better than a fortnight, though without occasioning to the patient the smallest uneasy sensation. In Mr. Grimes's case the dose of tincture was pushed to ninety-six drops without inconvenience, and his pulse fell to forty, yet no intermission was felt; nausea, however, and vomiting after his meals supervened the day after this quantity had been taken; these continued four or five days, and were only alleviated by the omission of the digitalis.

Here therefore, in one instance, one hundred

drops, in another ninety-six of the saturated tincture, were by gradually encreased doses, safely introduced into the system of very debilitated patients, before either sickness or irregularity of the circulation appeared, and even then these symptoms proved of little moment, as the first was speedily removed, and the second produced no inconvenience. During this period all the symptoms of irritation and fever, cough, pain, and dyspnœa daily grew better, and at length altogether retired. On the quantity and quality of the expectorated matter, the digitalis soon exerted a most remarkable effect, either promoting its absorption, or diminishing its secretion, or perhaps both, in a rapid manner, whilst at the same time it deprived it of its fœtor.

What, however, I consider as of most importance in these cases, and to which, perhaps, we are alone indebted for a cure, is the demonstration of the possibility of retarding the circulation for weeks together, by the use of this medicine. In Mr. Marris's case the pulse never rose beyond fifty from July 12th to Au-

guft 15th, nor in Mr. Grimes's from September 17th to October 8th ! How greatly every falutary purpofe, every curative intention, muft have been forwarded by this permanent depref-
 fion of the circulating powers, muft be obvious to every medical reader, nor will the limits of a letter allow me to expatiate farther on the fubject ; I fhall only add, that the theory laid down, and the facts now given, will, if I mif-
 take not, mutually illuftrate each other.

One circumftance of difparity in the two cafes as to the operation of the digitalis, fhould be mentioned ; it being neceffary with Mr. Marris, after the pulfe had funk to forty-four, daily to perfift in the ufe of a dofe of the tinc-
 ture, to maintain the depreflion, and one day when the pulfe was below fifty, from a wifh to afcertain the refult, the two dofes of the tincture were omitted, and the pulfe next morning beat 112, whereas in Mr. Grimes's cafe, though the digitalis was entirely omitted on the 24th of September, in confequence of the naufea, on the 2d of October the pulfe had not rifen beyond forty-eight.

The activity of this medicine is so great, that in cases where much debility is present, the constant attendance of a person well apprized of its mode of operation and effects, should be considered as absolutely requisite. Though Mr. Marris was at some distance from me, and I had not an opportunity of seeing him daily, yet was I free from any anxiety on that account, as the gentleman with whom he resided, the Rev. John Hildyard of Monks-Eleigh, was not only well versed in medical science, but paid the most unwearied attention, both to the progress of the symptoms, and the exhibition and effects of the tincture. To his well-directed assiduity I think myself much indebted for the success with which this attempt to cure consumption through the medium of digitalis has been accompanied.

I may, I think, without hesitation affirm, that an early exhibition of the saturated tincture in consumption, will in general prove successful, and even when the disease is far advanced, provided the patient has but strength sufficient left to endure a gradual depression of the cir-

culation, a result equally fortunate may be expected. That this can be done, even in circumstances of debility, to an extent adequate to effect a cure, and without either sickness, languor, or loss of appetite, the cases now appended will satisfactorily attest. I have only to wish, sir, they may contribute somewhat toward promoting the great and humane design in which you have so long and so laudably been engaged. I am, Sir, with great respect, &c.

NATHAN DRAKE.

Hadleigh, Suffolk, Feb. 21, 1799.

JAMES MARRIS, aged 16, complains of considerable difficulty of breathing upon motion, and of pain in the right side. He has a frequent short cough, attended with a copious expectoration of pus, dense, foetid and occasionally mixed with blood. Pulse 120, and the morning and evening exacerbations, especially the latter, strongly marked. Great ema-

ciation and prostration of strength. Tongue clean. Thirst not considerable. Appetite not much impaired. Belly regular. Sleep interrupted, and he cannot lie with ease on his left side. Slight perspiration toward morning. Skin hot and dry. Shivering fits every two or three days. Urine high-coloured.

He has ever been of a delicate constitution, and for two years or more liable to dyspnoea and pain in his chest on using exercise, accompanied by a spitting of blood, and slight purulent expectoration. An hereditary tendency to phthisis exists in the family, his mother, aunt, and uncle having perished under that complaint. He is evidently likewise of the form and habit which physiologists consider as predisposing to tubercular consumption. Having a few months ago removed from his native county of Lincolnshire to a high situation in Suffolk, and shortly afterwards exposed himself to cold whilst fishing, he was attacked with a return of hæmoptysis attended with stitches in his side, and fever. These symptoms were mitigated under the care of a neighbouring surgeon, and

he continued for a short time tolerably well, but upon his difficulty of breathing, pain and fever recurring, though in a slight degree, he removed to Monks-Eleigh, beneath the care of a near relation. In about a week after this change of situation, his dyspnœa and cough encreased, and he now began copiously to expectorate what appeared, and upon trial, with the vitriolic acid and caustic alkali, proved to be in a great proportion pus. This and the other symptoms still augmenting, my assistance was required, and I saw him on June 22d, 1797, and had six ounces of blood taken from his arm, which on cooling proved considerable fizy. I ordered a blister to be applied to his right side, and prescribed some pills composed of myrrh and antimonial tartar, to be taken with neutral mixture three or four times a day.

June 26. Pulse still 120, with great heat, difficulty of breathing and cough. Expectoration, if any thing, encreased, and very foetid. Pain rather mitigated: Great debility, so as to be incapable of walking across the room without assistance. Little or no sleep, and slight perspirations towards morning.

R. Foliorum digitalis purpureæ in pulverem crassum. trit. unc. i.

Spiritus vini rectificati et

Aquæ puræ aa unc ii.

Digere leni calore, sæpe agitans, per horas xxiv. et cola.

R. Kali pp scrup. i.

Succi limonis unc. ss.

Aquæ puræ drachm. vi.

Tincturæ digitalis purp. gutt. xv.

fiat haustus primo mane et horis duabus ante prandium fumendus.

Ordered to take asses milk morning and evening, and allowed a little wine and animal food. June 28.—Pulse 108; cough, breathing and pain rather easier. Debility appears to be encreasing. Expectoration extremely copious and heavy. He is confined from weakness to his bed and couch, and his friends apprehend him not capable of surviving many days. No sickness however from the digitalis. The tincture ordered to be encreased to twenty-five drops in each draught; more animal food allowed.—July 1st. Pain nearly gone; expectoration rather diminished; cough not so frequent; breathing much freer; debility not encreased since the last visit; skin cooler. Ordered thirty drops of the tincture in each draught.—July 3. Pulse 80; breathing continues much easier; expectoration evidently

diminished, and the foetor abating. He is capable of more exertion, and feels himself in every respect better, except that his appetite begins to fail. Ordered to take his draughts as on the 1st.—July 5. Pulse 76; skin of the natural temperature, body open; no increase of urine; expectoration greatly diminished; breathing free, but still unable to walk without assistance; no sickness from the digitalis, but appetite much impaired.

R. Cinchonæ in pulverem tritæ unc. i.

Quassia ligni et.

Corticis aurantii aa drachm. ii.

Aquæ ferventis unc. xii.

Macera per horas tres et cola.

R. Infusi colati unc. iss.

Acidi vitriolici diluti guttas x.

Tinct. digital. purp. gutt. xxxv.

Fiat haustus mane et meridiæ sumendus.

July 8.—Pulse 68. Appetite improved, being able to take much more animal food, and three glasses of wine per day; breathing easy; skin natural; expectoration rapidly diminishing. Ordered to take forty drops of the tincture in each draught twice a day.—July 12. Pulse 50. Cough nearly gone; breathing perfectly easy; sleep sound; expectoration decreased

more than two thirds ; ordered fifty drops in each draught.—July 17. Pulse 44 ; expectoration amounting but to three or four table-spoonful ; no cough ; strength daily encreasing, and he is able to walk about the house ; appetite continues good ; much intermission in the pulse ; draughts twice a day as on the 12th. July 22.—Pulse 40, and intermits every third stroke ; no sickness ; expectoration amounts but to one table-spoonful, and when tried with the vitriolic acid evinces very little matter. No cough, no difficulty of breathing ; appetite remains good, and his strength continues to encrease ; ordered to take only forty drops of the tincture in each draught.—July 28. Pulse varying from 44 to 48, and still intermitting ; strength so much restored as to be able to walk out for some time during the middle of the day. Other symptoms as on the 22d. Ordered but thirty drops in each draught.—August 4, Pulse 48 and sometimes 50 ; intermits every sixth or seventh stroke ; strength almost restored, and he is rapidly gaining flesh. Ordered but twenty-five drops of the tincture in each draught.—

Aug. 15. His pulse has now never risen beyond 50 for better than four weeks ; his strength is almost perfectly restored, and he can ride out for eight or ten miles with but little fatigue or uneasiness of any kind. His cough and difficulty of breathing are entirely removed, and his expectoration, which is now a mere trifle, is altogether free from pus. He feels no pain even from a deep inspiration, and his countenance has a healthy appearance. Ordered to omit the digitalis, but continue the infusion of cortex and quassia twice a day.—August 29, Pulse 70 and free from intermission ; strength perfectly restored ; flesh nearly regained, and in all respects indeed well.

CASE II. September 10, 1797. GEORGE GRIMES, aged 19, complains of very acute pain in his right side, which is increased by inspiration ; he has incessant cough, and great difficulty of breathing, accompanied with frequent expectoration, which is evidently purulent, and very foetid. Pulse 120, and hard. Complexion very florid ; tongue foul ; thirst

great ; appetite much impaired ; body regular ; urine high-coloured, and depositing a copious sediment ; little or no rest ; has frequent shivering fits, and his health is rapidly declining. He has been liable for many months to slight pulmonary complaints, as cough, hectic flushings, and occasional expectoration ; and has been for some weeks taking cicuta under medical direction in town. It being thought advisable for him to try his native air, he arrived in Hadleigh about two months ago, and conceived himself for some time better from the change of situation. A few days ago, however, he was suddenly attacked with the symptoms above enumerated, after exposure to wet and cold ; and to alleviate which he has been twice bled and blistered by his surgeon, though without any material relief. It is necessary also to observe, that his father, mother, and sister, all died of phthisis pulmonalis in this place ; the two latter I attended under this disease about five years ago.

In consequence of the good effects I had experienced in the use of digitalis in Mr. Marris's case, I prescribed as follows.

R. Kali pp. scrup. i.

Succi limonis q. s.

Lactis amygdalæ unc. i.

Tinct. digitalis purp. gutt. xx.

Fiat haustus mane et meridiæ fumendus.

R. Mucilaginis sem. cydonii mali et

Aquæ cinnam. unc. iii. m. et sumat cochleare largum urgente tussu.

Sept. 11th. Pulse 112 and softer; cough very troublesome; breathing rather easier; expectoration very copious; skin hot, and urine high-coloured; pain still violent. Ordered twenty-five drops of tinctura digitalis in each draught. 12th. Pulse 108; pain of the side abated; cough easier, and breathing more free; expectoration not quite so copious, and nearly free from fœtor; skin cooler. Body continues regular; tongue cleaner. Ordered thirty drops of tincture in each draught. 13th. Pulse 94; expectoration diminishing; pain nearly gone; breathing easy; thirst abated, and appetite returning; cough greatly better. Ordered thirty-five drops of tincture in each draught. 14th. Pulse 80, soft and regular; expectoration reduced to half the quantity; pain entirely removed; slight perspiration during the night; ordered forty drops of the tincture in each

draught. 16th. Pulse 64; expectoration continues rapidly decreasing; cough nearly gone; no thirst; tongue clean and natural; body regular; appetite good; perspiration at night more profuse. Draughts as on the 14th. 17th. Pulse from 50 to 56. Other symptoms as on the preceding day. Ordered forty-five drops in each draught. 18th. Pulse 50, and regular; expectoration amounts but to four table spoonsful; skin cool; cough perfectly removed; strength manifestly increased. Perspirations during the night continue. Draughts with digitalis as yesterday:

R- Decocti cinchonæ unc. iſs.

Acidi vitriol. diluti gutt. xxv.

Fiat haustus omni nocte horâ somni sumendus.

Sept. 19th. Pulse 48. Other symptoms as yesterday. Repetantur haustus. 21st. Pulse 48; expectoration has no purulent appearance, and amounts but to three table spoonsful; perspiration at night much abated; appetite much impaired.

R. Cinchonæ pulv. unc. i.

Quassia drachm. ii.

Aquæ ferventis unc. xii.

Macera et post horas iv cola.

R. Infusi colati unc. ifs.

Tinct. cort. aurantii drachm ii.

— digitalis purp. gutt. xxxvij.

Fiat haustus mane et meridiæ sumendus.

22d. Pulse 40, and regular ; some nausea ; little or no appetite ; nightly perspirations gone. Expectoration as on the 21st. Repetantur haustus. 24th. Pulse 40, and regular ; nausea increased, and yesterday and to-day he has brought up his food in a few minutes after swallowing it ; expectoration to the quantity of about a table spoonful ; body rather bound.

Omittantur haustus cum decocto cinchonæ, &c.

R. Lactis amygdalæ unc. i.

Sp. nucis moschatæ drachm i.

Tinct. digitalis p. gutt. 50.

Fiat haustus primo mane sumendus.

26th. Pulse 40, and regular ; nausea continues, and his stomach is unable to retain food. Expectoration entirely free from pus ; no motion for the last four days. Omittantur haustus cum tinctura digit.

R. Decocti pro enemate unc. xii.

Natri vitriolati, et

Olei olivæ aa unc ifs.

Fiat enema statim injiciendum.

R. Emplastri ladani drachm vi.

Opium purificati, et

Olei macis per expressionem aa drachm i.

Fiat emplastrum regioni ventriculi applicandum.

28th. Pulse 44, and regular; the stomach retains food, though the nausea in a slight degree continues. Enema operated well.

R. Decocti cinchonæ drachm x.

Tinct. cinchonæ drachm i.

Acidi vitriol. dil. gutt. x.

Fiat haustus horis duabus ante prand. vespertique sum.

29th. Pulse 44, and regular; nausea gone, and appetite perfectly restored. Pergat in usu haustum. October 2d. Pulse 48, regular and soft; strength greatly increased; breathing perfectly easy. About a spoonful of mucus evacuated daily, and without any mixture of pus; body open; appetite good, and in all respects free from complaint. 8th. Continued well until yesterday, when remaining stationary in the garden for about two hours during the middle of the day, he caught cold. Pulse 108; skin hot; face flushed; breathing quick and laborious.

R. Lactis amygdalæ unc. i.

Sp. nucis mosch. drachm i.

Tinct. digit. purp. gutt. xx.

Fiat haustus hac nocte horâ somni sumendus, et cras primo mane repetendus.

9th. Pulse 72; skin cooler; breathing easy; suffusion on the face gone. Repetantur haustus

cum tincturæ guttis xxv. singulis. 10th. Pulse 60; skin cool; breathing easy; appetite good. Cont. haustus. 12th. Pulse 48. In all respects well. Ordered to take only one draught with twenty-five drops of the tincture of digitalis every morning. 19th. Continued well, and with a pulse varying from forty-eight to sixty; and taking the draught every morning until to day, when he was attacked with a purging, and has had five loose motions. In other respects well.

Omittatur haustus cum tinctura digit.

R. Misturæ cretaceæ unc. vii.

Sp. Lavend. comp. unc. fs.

Tinct. opii. gutt. xxv.

Fiat mistura cujus sumat cochlearia tria quater de die.

Ordered to omit vegetables and malt-liquor: Oct. 21st. Pulse 56, and regular; diarrhœa abated. Repetatur mistura. 27th. Pulse 64; diarrhœa gone. In all respects well:

N. B. The diet, after his pulse had sunk to 50, was chiefly composed of animal food, and he drank three or four glassses of wine per day, with porter at his meals.

Letter from Dr. FOWLER, on the cure of
CONSUMPTION.

I send you (to dispose of as you may think proper) the result of most of the trials which I have hitherto made of the digitalis in cases of pulmonary consumption. As I began to give it more from a dissatisfaction with the remedies usually employed in this disease, than from any very sanguine expectation of success, I took no notes of many of the cases in which I first used it. With respect to these I can therefore supply you with little more than the dose of the medicine, and its more prominent effects.

Observing, however, that it in no case did harm ; that on the contrary, it almost uniformly relieved the most distressing symptoms of the disease ; and that in some it appeared even to have effected a cure, I began to collect as carefully as I could, all the material circumstances of the cases in which it had been given ;

and the instances of its good effects, which I am now to lay before you, will I hope, induce you to avail yourself of the many opportunities which must occur to you, for giving this subject a full and accurate investigation.

As I had frequently seen large doses of the digitalis given by others, and had myself still more frequently given it in dropical cases, without ever observing any of those uncontrollable and dangerous effects,* which are said to deter many from its use, my mind was perfectly at ease as to its probable effects in phthisis, and the more so as its power of repressing arterial action, and inducing debility, from which we have most to apprehend in dropsy, was the very quality, from which properly directed, I hoped to derive most advantage here.

My attention was indeed first directed to it as a remedy likely to be useful in phthisis, by

* I would by no means be understood to dispute its power of producing deleterious and fatal effects, when given in quantities larger than what is warranted by (now ample) experience. This power I know it has; but it has it in common with many others of our most valuable remedies;—and no one surely would reject a good, because from its abuse it might possibly be productive of evil.

its almost uniform effect of rendering the action of the arteries more flow than natural, at the same time that it appears to excite that of the absorbents. It has long been known, that diseased parts of the body may be removed by depriving them of *all* supply of blood from the arteries ; and it is now known, that where this cannot with safety be attempted to so full an extent, on account of the intimate connection subsisting between the part to be removed, and such as we wish should remain, that the same effect may be produced by diminishing to a certain degree the arterial supply of the part, at the same time that we leave the action of the absorbents in full force. This is the purpose so ably effected by Mr. Hunter's scientific operation for the cure of popliteal aneurism : and I confess that I was not, and that I still am not without hope, that something analogous to this may be effected by the operation of digitalis on tubercles in the substance of the lungs. But my expectations of success had a better foundation than reasoning a priori.

There was good ground to believe that Dr.

Darwin had cured one case * of phthisis by a strong decoction of digitalis.

Dr. Ferriar had certainly cured by an infusion of this plant, four cases of hæmoptoe, a disease nearly connected with consumption ; and in a note to page 18 of his 2d. vol. he says expressly, that he has “ repeatedly stopped the progress of incipient consumption by administering digitalis, when the patient was too much weakened by preceding disease to bear the usual methods of lessening the impetus of the circulating system.”

Dr. Withering's opinion of it (notwithstanding his expressed wish that it may be further tried in this disease) was not, it is true, very encouraging ; but it should be recollected, that even with him it succeeded completely in one case, (No. cxx.) that it relieved another, (No. xl.) very far advanced, and that the remaining cases in which it was given by him were lost before recourse was had to the digitalis.

* I did not then know what the Dr. has since told us in his *Zoonomia*, vol. 2, p. 291 ; that this case (much relieved for a time) terminated fatally. He there relates another case certainly cured by it, but is in doubt whether this were not peripneumonia, or catarrh.

CASE I. The first case of consumption, in which I had an opportunity of observing the effects of digitalis, was in a girl received into the Stafford Infirmary, under the care of my friend Dr. Edward Alexander, at the beginning of the year 1794. I have no notes of this case, but I perfectly recollect, that her symptoms impressed both of us with the opinion of its being incipient consumption. Her cough was particularly troublesome, and her pulse very quick. The suggestion of the digitalis, I believe, came from me. She took it in powder, I think gr. i. two or three times a day. Her recovery was so rapid, and apparently complete, as not a little to please and surprize both of us. What afterwards became of her I had no opportunity of knowing.

The following cases of out-patients, for whom I had prescribed the digitalis at the Salisbury infirmary, are by no means so full and detailed as I could wish them; but the unfavourable circumstances under which medicines are administered to this very indigent class of patients, their exposure to causes perpetually counter-

acting the effects of remedies, and renewing their complaints, together with the irregularity of their attendance, held out but little encouragement to note down the history of each individual's disease, at the time I began to give the digitalis. Finding, however, that all of them gave a favourable report of the effects of this remedy, that it uniformly freed them from the sensation of oppressive tightness about the chest, quieted their cough, rendered the pulse more slow, and wherever hectic fever and profuse night sweats had taken place, put a stop to them; I endeavoured to aid my own recollection of them by the best accounts which I could collect from themselves. The doses of the remedies, and the periods during which they were used, are accurately transcribed from the books of the Infirmary.

CASE II. GEORGE MATTHEWS, aged 12, of a florid complexion, employed in the carpet manufactory at Wilton (a fertile source of diseases of the chest), was admitted an out patient of the Salisbury Infirmary, May 20, 1797. Symptoms, a violent, constant, and

dry cough of some standing ; pain and excessive tightness about the chest ; wheezing ; shiverings, succeeded by flushings of heat every morning, and mostly every morning after breakfast. Pulse very frequent. Having kept him low, blistered him, and given him a saline mixture in the act of effervescence, till July 10, and with very little relief of his complaints, I ordered him to take unc. fs. of the following decoction of digitalis every six hours till it should produce sickness ; then to take it only once a day.—Folior. digitalis purpureæ recentium unc. ii. coque ex aq. puræ lb. i. ad colaturæ unc. viiss, et adde tinct. cardamom. unc. fs. On the 17th, I find it had not made him sick : he was desired still to continue it, and a blister was applied to his side. On the —, the digitalis was discontinued, and a decoct. cinchonæ cum acid. vitriol. dilut. was directed, together with opii. gr. i. omni nocte. These last medicines were again repeated in November, after which I heard no more of him till January 1st, 1799, when he returned and informed me that he had continued perfectly well during the year

he had been absent, but that on catching a fresh cold, all his former symptoms had returned. I again gave him decoct. digital.* unc. fs. bis quotidie. He did not come again to the hospital till the 29th, when I found he had taken only a pap spoonful twice a day ; but said the tightness across his chest, pain, and hoarseness, were much relieved. His cough was still troublesome. He was desired to continue his medicine, but I have seen nothing of him, the distance and severity of the weather having prevented his coming.

CASE III. Is of a man to whom emetics, salines, and blisters, had afforded but little relief. I gave, while at the infirmary, unc. i. decoct. digital. His report of its effects at the next visit were so favourable, that I gave him unc. fs sextis horis, with succi spissat. cicutide gr. iii. omni nocte. Five days afterwards he made no complaint but of weakness. I therefore gave him the following medicine, in which I find him persisting at the time of his last visit,

* During the winter months, the apothecary, I find, has always made it with the dried leaves.

October 9, 1798.—Kali pp. drachm. iifs, decoct. cinchonæ unc. viifs, tinct. ejusdem drachm. iv. m. fumatur unc. i. ter quotidie adjiciendo succi limon. unc. fs.

CASE IV. MARY WATERS, July 28th 1798. Of her I know no more than what I find in the hospital book. Sumat. decoct. digital. unc. fs bis in die: admov. emplast. cantharid. lateri Aug. 11. decoct. digit. unc. i, decoct. cinchonæ unc. v. tinct. opii. gutt. xl. m. fum. unc. fs, ter in die-24, pergat. Though I saw no more of her, yet from my custom of never ordering the bark while any difficulty of breathing continues, and from the circumstance of this order having been repeated in this case, I think it may be safely concluded, that she found herself well.

CASE V. ANN HUNT, aged 17, Aug. 11, 1798. On a superficial examination, this first appeared to be one of those cases which Dr. Withering notices as so difficult to be distinguished from chlorosis. The paleness of her lips contributed to mislead me. Steel and bitters aggravated all her symptoms; but blif-

ters, with decoct. digit. unc. fs bis in die, soon perfectly cured her.

CASE VI. ELIZABETH GEORGE, Sept. 8, 1798. After a blister and decoct. digital. unc. fs. bis in die till 11th, decoct. of bark was combined with it. And on Nov. 3, after which I saw no more of her, I find her taking a mixture of myrrh and steel.

CASE VII. SAMUEL JAMES, aged 32, tall, thin, and pale, with a long neck, narrow chest, and high shoulders: for some years past a wool comber. While at Gibraltar, about nine years ago, had a cough with much white expectoration, for which he went to the hospital, and was cured in a month by emetics. His cough, with pain in his side, difficult respiration, and every symptom of confirmed phthisis, again returned about a year and a half since, and continued increasing till Oct. 6, 1798, when he applied to me. I ordered decoct. digit. unc. fs. bis in die-23. he was directed to continue it every eighth hour. This he did regularly till Jan. 1st, 1799, when I found him in every respect relieved; but as he was not

quite free from complaint, I ordered an infusion of digitalis (unc. i. to aq. fervent. unc. viii.) with the addition of tinct. cinchonæ unc. fs, pulv. ejusdem drachm ii. On the 29th he made no complaint but of dyspeptic symptoms, which were soon removed by rubig. ferri gr. viii. quassia pulv. gr. xii. bis terve in die. I have since seen him two or three times, and he continues, notwithstanding the season, perfectly well.

CASE VIII. RICHARD SMART, aged 14 : Nov. 1799. This boy had every symptom of phthisis in a state of rapid progression. A blister was applied to his chest on account of acute darting pains there, and unc. fs of decoct. digital. was directed to be taken twice a day. On the 20th being much relieved, he was desired to continue the same dose. His pulse at the next visit, was found reduced to 68, and was rather irregular. His appetite, however, remained good, and he slept well. The digitalis was omitted, and decoct. cinchonæ, with the addition of kali and lemon juice in a state of effervescence, substituted. On the 22d of

December, nearly well : continue. January 1st, 1799, much better : continue 22d: Apparently well ; but I thought it right that he should continue the effervescing bark mixture a little longer. I have seen no more of him.

CASE IX. JOSEPH YOUNG, a labourer, aged 50, admitted January 15, 1799 : said he did not consider himself very ill till about christmas, when on coughing, something seemed to give way within him, and he spit up a great quantity of very foetid matter. I gave him the decoct. digital. unc. fs, ter in die, till the 19th, when having been made sick by it, and being on the whole considerably better, he was directed to take it only once a day till the 26th. Being now much better, it was laid aside altogether, and a decoct. of bark substituted. This he continued taking without any return of consumptive symptoms on the 19th of February, when I last saw him, in good spirits.

I have still several out-patients lately admitted, whom I am treating in the same manner with those whose cases I have detailed ; and at present I see no reason to be dissatisfied with the effects of my plan.

CASES of IN-PATIENTS.

THOMAS FITZGERALD, an Irishman, aged 32, admitted an in-patient of the Salisbury Infirmary, July 13th, 1798. He said he had been ill ten months, the three last of which he had passed in Hasler Hospital, from which he had been discharged as incurable. His appearance was so much that of a man who had not many days to live, that I refused to decide on his admission, till requested by some of the governors to admit him. His complexion was very dark, his countenance haggard; his figure tall and extremely emaciated. His neck was very long, his shoulders high, and his chest narrow. His pulse was small, hard, and 130. He complained much of pains in his chest shooting through to his back. For this he had often been blistered, but without deriving any relief. He coughed incessantly, and his expectoration which was copious, had every appearance of pus. He had regularly two accessions

of hectic fever every day. His legs had begun to swell but not to any considerable degree.

The symptom which engaged this man's attention most, was oppressive tightness across his chest; and it deserves to be remarked, that this was so much relieved in the course of a few hours after taking the first half ounce of the decoction of digitalis, that he told his nurse he felt perfectly well, and thought he had no occasion to trouble the charity any longer. He continued to take unc. ss. decoct. digit. quartis horis upwards of a week, without its either producing sickness, or rendering his pulse at all slower. Every other symptom had abated. An issue was directed to be formed in the part of his chest where most pain had been felt, and the digitalis was continued as before till the 28th, when he was so much better, that I gave him the decoct. of bark, and ordered the digitalis to be taken twice only in the day. On the 14th of August, his pulse was perfectly natural, his cough and fever had left him, and he made no complaint. He remained a week or two

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longer with us—recovered his flesh, and considered himself as perfectly well.

CATHARINE MUSSELWATE, with symptoms of phthisis, admitted an in-patient, July 14. Took ipecac. pulv. scrup. i. On the 17th, began to take decoct. digital. unc. ss. sextis horis: On the second or third day was very sick. The digitalis was now changed for the decoct. cinchonæ with kali and lemon juice in a state of effervescence, as mentioned in former cases. On the 31st. this was changed for Griffiths mixture of myrrh: ferrum vitriol. &c. On the 27th of August, I find I had again recourse to decoct. of bark, but now with vitriol. acid, and on the 15th of September she was dismissed cured.

SARAH ALEXANDER, admitted July 30th. She took decoct. digit. bis die till Aug. 4th, when having been made rather sick by it, and all the symptoms for which it was given having abated, I directed for her the bark effervescing mixture as above, and on the 7th, a blister on her chest. On the 19th Griffiths's mixture, and shortly afterwards she was dismissed cured.

THO. GIBBS and JOS. WHITE, both received cures by nearly similar means.

MARG. NAYLOR, aged 36, a servant, about 12 years ago, after the usual symptoms of hæmoptoe, spit a very large quantity of blood. About a year afterwards, on coughing, she bled profusely from the nose. This very frequently returned in excess; and as she did not menstruate, the late Dr. Jacob ordered her to be bled, which she was every five or six months, during three years. On neglecting to be bled, cold shiverings, followed by profuse sweats, attacked her; and she has ever since had cough, pain in some part or other of her chest, a sense of fulness and oppression there, and a quick pulse. I saw her for the first time in October last. During the six weeks previous to this, she had been more than ever tormented by acute pains darting through her chest and between her shoulders, attended with shortness of breath and constant fever. For a considerable time she had scarcely been able to lie down, and had had no sleep. Her pulse at this time beat 140 in a minute. I gave her Dr. Darwin's

decoct. digit. which produced excessive sickness, and after the first day relieved entirely her pains and difficulty of respiration. I now gave her opium in small doses for about a week, and she became freer from complaint of every kind than she had been for years before. She has lately had another attack of the same kind, but aggravated by an evident effusion of water externally, and every symptom of its affusion into the substance of the lungs. Digitalis again relieved all her complaints, and I saw her to-day in what she considers as very tolerable health.

Sometime about April last, I was consulted by the butler of a family in which I was attending. He appeared to be about 40, had lived freely and irregularly in every respect; he had every symptom of an incipient phthisis. As I knew I should seldom have an opportunity of seeing him, I did not venture to give him digitalis, but desired him to live as much as possible on milk, and to use as common drink a saline mixture in the state of effervescence. I saw no more of him till the 18th of

September, when his cough and all other symptoms had very much increased, and what he spat was yellow, and of a very disagreeable taste. I now gave him (as he was to be near me) unc. fs. of a decoction of digitalis, made by boiling unc. i. of the fresh leaves in unc. viii. of water, till reduced to unc. v. and desired him to repeat the dose every eight hours till sick. As it had not produced this affection the 21st, I desired unc. fs of a rather stronger decoction to be taken every four hours ; after the fifth dose he vomited, and continued to do so (though the medicine was discontinued), for three days. All his symptoms were not gradually, but almost instantly relieved. . Opium in small doses, as mentioned by you in the *medical facts and obs.* put a stop to his sickness, and he continued to take it during several weeks, on account of a return of cough. Some months have now elapsed since I saw him ; and a companion of his informs me, that he has become the father of a child, is married, and so far as he knows, in tolerable health.

A gentleman, who for some years had been tormented with pain in his chest, and at different times had spit large quantities of matter, applied to me last summer with every appearance of confirmed phthisis, combined with dropical symptoms, particularly those of hydrothorax. As I was not often to have an opportunity of seeing him, I gave him decoct. digit. unc. ss ter quotidie, till sickness was induced. It relieved all his symptoms in a few days, and in a month he was capable of conducting a business requiring considerable activity, and considered himself better than he had been since his first attack. He died lately, and so far as I can learn from his son, in consequence of a sudden effusion of water within the chest. I have every reason to believe that he was a very intemperate man, and that he died a victim to his own imprudence.

The following case I think you will consider with me as particularly valuable. I regret that I have no notes of it, and must relate it wholly from memory. A servant girl aged 20, tall, fair, and of that form which at first sight leads

one to apprehend consumption, complained to me last May, of great heat in the palms of her hands, irregular shiverings, cough, pain in her chest, and oppressed breathing. Her pulse was small, hard, and very frequent. These symptoms had continued some months, but not always in the same degree. The bark, which she had taken from an idea that her complaints arose from weakness, she told me, had increased her heat, cough, and tightness across her chest in a very alarming manner. I gave her the digitalis in the following manner:—
Folior. digital. recen. unc. fs, coque ex. aq. puræ unc. v, ad colaturæ iifs, et adde tinct. cardamoni unc. fs, sumatur unc. fs, bis quotidie. It occasioned a very slight degree of sickness, and in the course of a fortnight freed her from all her complaints except of weakness, which was in a short time relieved by decoction of bark in a saline draught. She has ever since continued perfectly well. Several of her family had died consumptive.

I was desired in July last to see the wife of a publican, aged 40 ; she had been a healthy wo-

man, and was the mother of several children. She complained of acute pains in different parts of her chest, and a sense of great tightness there; coughed a great deal, and spit what had every appearance of matter tinged with blood. Her pulse was very frequent, and she had regular paroxysms of a hectic fever, with profuse sweats towards morning: her legs were œdematous, and she made but little water, of a high color. These symptoms had been gradually increasing during two months, and were attributed to a cold caught while lying-in. I gave her Dr. Darwin's decoction of digitalis unc. ss quartis horis, with opii gr. i. scillæ gr. iiss, every night. She was soon freed from all her bad symptoms, and Griffiths's mixture made effervescent, soon restored her strength and spirits, and she is now, and has been many months, in perfect health. Call this case what you please, I am aware that not only it, but several of the others require comments, but I have not at present time to make them, nor even to read over what I have now written.

R. FOWLER.

To DR. BEDDOES.

Addition by the EDITOR.

I had often given digitalis in small doses ; and perhaps there are few physicians in the kingdom who have not trifled with this medicine in consumption. In consequence of a letter from Dr. R. Fowler, received about three months ago, I returned to its use, under a conviction that neither my previous want of success, nor that of others, formed any objection to its efficacy, when administered with greater perseverance, and in larger doses.

A good many of the cases in which I have hitherto tried it in effective doses, were *lost cases*, that is, the patients were in the latter part of the last stage of consumption. I have not yet been fortunate enough to rescue any patient in such a situation. But in most instances there was a great alleviation of symptoms ; in none did life appear to be shortened by the medicine ; in some, as far analogy enables me to judge, it was greatly protracted.

What I observed principally in such desperate cases was as follows. I do not conceive a minute journal of each can answer any useful purpose. Mr. ———, with black hair and dark eyes, had, as I collected from his account, contracted pulmonary consumption from exposure to cold. On his arrival at Clifton, a violent hæmorrhage from the lungs came on, and was succeeded by several others in the course of a few days. *Ceruffa acetata* was largely administered; and during this time, the cough and hectic fever were as usual suspended. About the time of the last attack of hæmorrhage, I received Dr. Fowler's notice of his success with *digitalis*; and I prescribed the decoction of the recent leaves in doses of half an ounce, sometimes twice, and sometimes four times in the twenty-four hours. The patient survived five weeks, and seemed to die of inanition. He never had any return of rigor, heat, or night sweat; the cough, which is commonly aggravated on the cessation of hæmorrhage, was moderate, and expectoration easy. He was not made sick; his pulse was never brought below 70.

Mr. ———, just able to travel, and obliged to take to his bed shortly after his arrival; with light hair and eyes, took the decoction in doses sometimes of two, sometimes of four drams. His night-sweats were less, and his breathing, as he thought, easier. I was afraid, as in the former case, of inducing sickness. Neither patient had diarrhœa. Three other persons, in nearly the same stage of the disease, took the decoction without apparent alteration. But I employed it with great caution.

A lady with light hair and eyes, in the beginning of the last stage of consumption, that is, with cough, hectic fever, purulent expectoration, her feet just beginning to swell at night, and stools occasionally loose and frequent, took half an ounce of decoction of digitalis every six hours; the seventh dose excited violent bilious vomiting, with no preceding diminution of the frequency of the pulse. Her expectoration had been very copious, and a large quantity of frothy mucus was always mixed with the purulent matter. The morning after the sickness, there was not in the expectoration a quantity of

frothy mucus equal in size to a pin's head. The wheezing, which had been very troublesome, ceased ; and the swelling of the feet abated. Before the sickness went off, the pulse fell from 112 to 70, and became irregular. For a month she took the decoction in doses of one, two, or four drachms, twice or thrice in 24 hours. The pulse never arose to 90 but once. The expectoration diminished at first, but afterwards increased while the pulse was about 70 and occasional nausea was felt. Twice vomitings of bile occurred, and after each there was a diminution of the expectoration. The night-sweats returned no more. The rigors and heats, which every day for many months had come on in the morning or about noon, were never once felt. The cough was much diminished. The general state of the patient was oppressive languor ; and I often questioned within myself whether she suffered greatly less from this languor than from the violent cough and strong hectic fever, which it had superseded. This patient had, except within a few hours of vomiting, an increased appetite under the digi-

talís. She expired placidly, after complaining greatly for a few days before of shortness of breath. She survived the first administration of digitalis near two months. During the latter three weeks she took tincture of digitalis, as recommended by Dr. Drake. But she could never take 30 drops without nausea, and 20 twice a day kept the pulse at about 70, though not without great languor. Under the use of digitalis the patient slept much more than before, without any increase of opium. The state of the bowels seemed little altered by this medicine. The chalk mixture was sometimes necessary.

A lady with light hair and eyes had expectoration of purulent matter with mucus; pulse 108—120. When I saw her there was no symptom of hectic fever, except pretty frequent night-sweats; but she had great pain of the bowels, with six, seven, or more lax stools in the 24 hours. She had long taken from 200 to 300 drops of laudanum daily. Extract of logwood largely given, with a diet of rice milk and hartshorn jelly without admixture, perma-

nently reduced the stools to one, two, or at most three in a day, and much abated the pain. Early in Jan. 1799, she began to take of decoction of digitalis half an ounce every six hours, the laudanum being continued as usual. The seventh dose produced bilious vomiting, which was followed by a reduction of the pulse to 76, with considerable irregularity. After two days suspension, the digitalis was continued in doses so regulated as not to produce vomiting. Loss of appetite, with great languor, and almost constant sleep followed, and continued a fortnight, but with no further reduction of the pulse, or of the expectoration. She was sometimes nauseated, and then always complained of pain in the bowels; but no increase of diarrhœa took place. The medicine being discontinued, the patient emerged from her torpor into a state of great ease and vivacity, attended with good appetite. Supposing I had given the medicine too freely, after a week's interval I ordered it again in a dose of one dram at the interval of eight hours; but nausea was twice distinctly produced by this cautious attempt to renew the trial of it.

After an interval of about a fortnight, an account of Dr. Drake's success with the tincture reached the ears of the patient's friends, who were determined that no chance of recovery should be left untried. A letter from Dr. Drake put me in possession of his method, according to which the medicine seems ingeniously opposed to the two exacerbations of hectic fever.

About this time the pulse had increased to above 100. The expectoration was as before the use of the decoction of digitalis. On Saturday the 16th of February, ten drops of the tincture were prescribed; and the medicine was raised by cautious steps to 34 drops twice a day, the laudanum being continued as before. The pulse was reduced to between 85 and 90. The second dose of 34 drops produced bilious vomiting, though no nausea (and hardly any languor) had preceded its administration. There was much more than usual sleep all this time, and the appetite had been excellent. There had been no night sweats. The sickness lasted two days, and the tincture was recommenced

in the dose of eight drops twice a day. For as there was no rigor, heat or other hectic symptom, there was no reason for administering the medicine at particular times of the day. It seemed to be adviseable to give as much as possible of the tincture without occasioning any disturbance of the stomach. The dose being gradually raised to 20 drops thrice a day, bilious vomiting was produced.

To shew the relation between the doses of the tincture and state of the pulse, I shall make a short extract from the journal of the case. I must premise, that though it now was proposed to give the digitalis thrice a day, yet if any considerable nausea was felt, only two doses were directed, or but one. I say *considerable nausea*, for not one of the following days passed without some nausea; and we had before proved that with fewer drops the pulse rose to near 100.

Sat. March 9th. 17 drops thrice in 24 hours.

Sunday 10th. the same. Expectoration lessened since Thursday, the day of bilious vomiting.

Monday 11th. 17 drops morning—20 afternoon—very sick after. Pulse 68 morning—100 afternoon—expectoration increased.

Tuesday 12th. 15 drops morning—ditto at 4 p. m. Pulse 86 morning—3 p. m. 68—midnight 80.

Wednesday 13th. 15 drops 5 a. m.—16 2 p. m.—16 midnight. Pulse 70 morning—2 p. m. 72—night 80.

Thursday 14th. 17 drops at 11 a. m.—ditto at 4 p. m.—ditto at 12. Pulse 72 morning—100 at night.

Friday 15th. 17 drops morning—ditto at 7 p. m.—ditto at 12.* Pulse 66 morning—night 78.

Saturday 16th. 17 drops morning—ditto at 7, p. m. and at 12. Pulse 70 morning—80 4 p. m.—80 night.

Sunday 17th. 19 drops morning—18 7 p. m.—ditto night. Pulse morning 80—ditto at night.

Monday 18th. 18 at 10 a. m.—18 7 p. m.—ditto at night. Pulse 86 morning—72 3 p. m.—98 at 12.

Tuesday 19th. 18 drops thrice. Pulse morning 80—at 2 p. m. 98.

All these days there was more or less nausea, though the appetite was good, and animal food, with one or two glasses of wine, taken twice. To day (19th) it has been without intermission, and was strong when the pulse was 98. The expectoration, after many variations, is nearly what it was before the tincture was begun: the cough has varied, but has been much better upon the whole than without the digitalis. The bowels have continued as after the relief

* I directed these doses so close together, because the pulse was 100 in the preceding night between 12 and 2.

from the logwood. Strength upon the whole diminished.

Rev. J. G. with black hair and dark eyes, had hæmoptoe about 18 months ago, which was succeeded by purulent expectoration and hectic fever. I saw him in May 1798, and advised a continuance of travelling, from which he had before experienced relief. In the course of the summer it at times nearly suspended all the symptoms. During winter they returned; and at the latter end of January the patient was greatly emaciated, had a fever-fit every evening, followed by night-sweats; his cough was severe, expectoration purulent, and he complained of pain under his left shoulder-blade, with some wheezing. Pulse 100—125.

I ordered half an ounce of decoction of digitalis four times in 24 hours, which in three days brought on bilious vomiting with excessive languor and greatly interrupted pulse, at about 80 in a minute, except during the evening exacerbation, when it rose to 95. After the sickness went off, the decoction was given in smaller doses, with intention to keep down the pulse

without nausea or languor. These, however, were brought on by a dose, which did not depress the pulse in the morning below 80 ; and it was at this rate greatly interrupted, and unequal in force. In the course of three weeks, the exacerbation, cough, and pain, continued much the same. There were no night-sweats. The legs had not begun to swell, nor did the patient complain of diarrhœa.

The tincture was now substituted in the dose of 15 drops twice a day, gradually augmented to 35 ; but this dose produced vomiting. I attempted to introduce more of the digitalis by dividing it into smaller doses, more frequently repeated. The patient, who was extremely intent upon giving the remedy a proper trial, carried the quantity of drops by gradual augmentation, up to 100 in the day : but bilious vomiting followed.

The pulse in the evening was never lower than 90, though it was near 60 before rising. The pain and uneasiness of the left side have rather increased ; the wheezing has become very troublesome ; the exacerbation is not less ;

the strength and flesh have declined : much torpor and unrefreshing sleep, without any opium, through the whole course. The appetite has been generally worse ; though the digitalis was sometimes taken in infusion of quassia, and sometimes in nitrous acid diluted. It was most agreeable in tea.

At the suggestion of the patient, who for many months had accurately attended to his own pulse, I made the following observation four different days ; and he told me I might have made it almost every day. The pulse being rather above 80, regular and weak, when he was sitting, he lay along the sofa for one, two, or three minutes, when his pulse became irregular, stronger, and slower, falling sometimes 20 strokes in a minute ; and upon raising himself slowly, it immediately returned to its former standard.

What is the cause of the difference between my success and that of my two able correspondents ? From Dr. Fowler's statement it is clear that it does not lie altogether in the period of the disease. I have suspected that at first when

I used the decoction, I prescribed it too largely ; yet when I again refer to Dr. Fowler's reports, I am obliged to reject this supposition. In the patient whose expectoration of mucus was stopped by an attack of severe sickness (a circumstance which inspired me with hopes of eventual success) the purulent part of the expectoration was diminished at each of the subsequent sicknesses : and the effect of sea-sickness upon many phthical patients, forbids me to conclude that the medicine failed from the too great severity of its action.

In late publications, a number of facts have been stated respecting the comparative operation of diuretic remedies. But as we before knew in gross the action of these remedies, and as there is little or nothing in the relation of the cases to enable us to decide *why* one succeeded when the other failed, it does not appear that our practice is rendered much more certain, or our theory more luminous : our knowledge is still in the gross as it was before. If the chances in physic were to be calculated like casts of the dice, these might be very good data. But hu-

man beings (in this respect unlike dice) have *interior* differences; and in his discernment here, are best seen the strength of the physician's philosophy, and the utility of his writings.

The preceding facts on both sides being allowed, what difference of result exists, must arise from the difference in the patients' constitutions, as affected by the disease, or anterior to its origin. Having rejected the first circumstance as inapplicable to *all* the facts, I shall endeavour to avail myself of the second for an explanation. I am unwilling to impute my failure to the use of opium, of which I tried in vain to get rid.

My patients were all slender, delicate, puny, or feeble. They had all been delicately educated, in which respect they must have differed widely from hospital patients. Now may I not assume (what in an *Essay on consumption* I shall immediately endeavour to prove by a copious induction of facts) that almost all the peculiarities in the mode of life among the more opulent classes, tend to lessen the contractile power of the muscular fibre; and certainly not less

than the rest, the contractility of the lymphatic vessels? If so, I believe it will be easily allowed that they will be less within the power of the association of motions, as well as of direct stimuli. I have long believed it to be a principle of the animal œconomy, that in weak habits the ordinary or natural connection between different sets of moving fibres, (or the irritative associations) are also weak. In such cases the stomach may be affected; the heart may be affected; yet the lymphatics of the lungs, which in less feeble or more irritable habits are excited into action, shall continue inert. The facts which I have related concerning the two last patients prove that the stomach was affected in all degrees, consistent with safety to life, by the digitalis; and yet that no adequate excitement of the absorbents was produced. I do not offer this opinion for acceptance, but merely to be compared with future facts. When Dr. Fowler favours us with those comments he gives reason to expect; perhaps he will help us to a better explanation.

Few people are better qualified for illustrating the actions of the animal œconomy.

In five cases of imminent or incipient consumption, the use of digitalis has either removed the complaint, or by producing the most decided good effects, affords hope of success.

A young lady, with light eyes and hair, of very feeble conformation, narrow chested, with elevated shoulder blades, and a very quick pulse, complained of a hard cough, which had succeeded to a short heaving cough, of shooting pains in the chest, and more fixed pain in the left side, shortness of breath, chilliness, and evening feverishness, succeeded by night-sweats. She had lately begun to expectorate, but I was not permitted to see the expectoration. Small doses of digitalis continued for three weeks, commonly at the rate of three grains of the powder in the 24 hours, removed all these symptoms. No sickness was produced; on the contrary, the appetite was restored under the use of the medicine.

Two other cases nearly similar terminated equally favourably: only that in these, sickness

was produced ; in one by twenty-eight drops of the tincture ; in the other by thirty-five ; and it was necessary to keep the doses between twenty and thirty.

The other two cases are in progress, and shall be fully related hereafter. In one the attack was by far the most severe I have ever known. After a hard cough of two months continuance, attended by decline of flesh and strength, constant indisposition, distinct evening fever-fit, with pulse at 130, but no expectoration, a most violent pain seized the left side, which rendered coughing excruciatingly painful. The patient's habit, weak pulse, and general loss of strength, appeared to me to prohibit general bleeding ; and topical was resisted. I trusted, therefore, to the tincture of digitalis. This in a month has removed the cough entirely ; has much reduced the evening exacerbation of fever, which in spite of two doses of the tincture, raises the pulse to 90, though it be 60 in the morning. The pain of the side continues, and mixes a good deal of anxiety with hope. It is however less.

The dose was gradually raised from 15 to 30 drops twice a day ; but 30 produced bilious vomiting. More than 20 cannot be taken without considerable nausea, vertigo, or indistinctness of vision. In all these cases except the first, I found it impossible to avoid great nausea, and to keep the pulse below 80. It would be 50 in the morning, and near 100 in the evening. I suspect that in people of feeble habit, the digitalis will lose its effect on the pulse sooner than in others ; and I suppose the above hypothesis applicable to the fact.

No other medicine, except an occasional aperient, was prescribed. Great sleepiness seemed the gradation between the ordinary state and depression. The patient observed that it was " the most sleepy thing she had ever taken."

The other case now in progress is probably not a case of tubercular consumption. The rest I take to be certainly so ; and I apprehend the great efficacy of the digitalis will be experienced in tubercular consumption.

An immediate emulation must be produced by these reports among medical observers. But

I will beg leave to suggest to them, that the determination of the powers of digitalis is not the only, perhaps after what has been already done, not the greatest, object of pursuit. We may presume that nothing stands alone in nature. And a substance of similar effect on the stomach and arterial system may lurk among the articles of the materia medica. It may be detected in some of the bitters, given in greater doses than common. It is impossible here not to think of chamomile which in a certain dose sickens, and of horehound which is the general domestic remedy in hæmoptysis and bad coughs. These and other substances given upon the above plan may exert virtues similar to those of digitalis. Chemical analysis may furnish light here, on which account I am glad Mr. Davy has engaged in the analysis of the digitalis. The discovery of an analogous body would add prodigiously to future success in the treatment of consumption.

ADVERTISEMENT.

The contents of this volume will shew that tolerably extensive tracts, as well as smaller papers, suit the editor's design. Such tracts will be thus better preserved, and perhaps be more widely circulated, than when published alone. If the collection goes on, I shall certainly not sacrifice utility to variety. I know nothing in authorship more detestable than the practice, so common in periodical works, of tantalizing readers by morselled information.

If the present volume appear somewhat later than was announced, its bulk and value will be a sufficient excuse for the short delay. Should the observations of Dr. Drake and Dr. Fowler be confirmed, it will undoubtedly prove the most valuable medical production ever offered to the British public.

I have reason to hope that the first part of a second volume may be ready by the end of the year. It will probably contain experiments on light, and on the respiration of gaseous oxyd of azote, by Mr. Davy—some matter by the editor—communications from Mr. Baynton and Mr. Creafer—observ. on the climate of Madeira, by a physician who has had good personal opportunities and excellent information.

My remarks on hospitals, whether right or wrong, will scandalize some readers. But I am very willing to assign to any paper, controverting my opinions and worth publication, the same station in the second which mine occupies in the first volume.

The application of the profits to public purposes may be an inducement with some to contribute to the undertaking.

T. B.

In two or three weeks will be published, by the EDITOR,

AN ESSAY

ON

PULMONARY CONSUMPTION,

For the use of families.

ERRATA.

Page	Line	
<u>44</u>	1	of the note, for 'a cubic' read 'an.'
<u>47</u>		strike out out all after line <u>7</u> .
<u>49</u>	1	for 'and' read 'though.'
<u>91</u>		for 'chromic' read 'chrome.'
<u>96</u>	1 & <u>5</u>	for 'phosnitric' read 'nitric-phos.'
<u>110</u>	<u>5</u>	of the note, for ' <u>2.25</u> ' read ' <u>1.85</u> '
111	<u>22</u>	of the note, for 'synthesis' read 'analysis.'
<u>112</u>	<u>6</u>	of the note, for ' <u>2.25</u> ' read ' <u>1.85</u> '
<u>143</u>	4	for 'azote' read 'nitrogen.'
<u>168</u>	<u>19</u>	for 'azotic' read 'nitric.'
<u>196</u>	15	omit 'and a superabundance of carbon.'
<u>276</u>	11	for 'circumstances. In' read 'circumstances, in'
<u>321</u>	1	dele 'of'
<u>475</u>	18	for ' <i>Family diet</i> ,' read ' <i>Family diæt</i> .'
<u>490</u>	1	for 'singulis' read 'singuli.'
<u>507</u>	18	for 'spiffut. cicutde' read 'spiffat: cicutæ.'
<u>508</u>	20	we should probably read 'one of those cases of consumption, which &c.'

Mere literal errors are left to the reader's correction.









